THE MEDICAL NEWS.

A WEEKLY JOURNAL OF MEDICAL SCIENCE.

VOL. LV. SATURDAY, OCTOBER 19, 1889. No. 16.

ORIGINAL ARTICLES.

THE RELATIONS OF ANÆMIA TO CHLOROSIS.[1]

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IN order to bring the whole subject up for discussion, as proposed by the Council of the Association, the referee and co-referee have thought it judicious to divide it into two sections. This has seemed the more proper as, upon mature consideration, compression of the more or less vague data into the allotted space and time seems almost impossible. To those who have occupied themselves with the pathology of the blood, and there are several in our midst whose names occupy a high position in this respect, the fact is apparent that confusion reigns supreme in this field which might, possibly, be easily cleared away if exact methods of examination were employed. Under all circumstances, our lines might be more strictly drawn and a beginning made for the more accurate and scientific study of these subjects; in this way elimination of one disease from another or identification would follow, and much benefit would accrue.

In the discussion of our present subject, the first thing that comes up to confuse us is the definition of the two diseases. Immermann (Ziemssen's *Hdbch.*, xiii. p. 275) begins his definition as follows: "The conception of anæmia, somewhat indefinite in its elasticity, etc.," and follows it up with nearly two pages of text, from which the final conclusion is to be drawn that, partly from clinical and partly from direct (analytical) evidence, we are justified in stating that anæmia is that condition in which there is a diminution of red corpuscles as well as of the albumins of the plasma in the blood (hypalbuminosis). Strümpell (*A Text-book of Medicine*, edited by Frederick C. Shattuck, A.M., M.D., p. 874) states that "the essential element in anæmia is, therefore, a diminution in the number of red corpuscles, or so-called oligocythæmia," and states further on that "oligocythæmia is not invariably accompanied by a diminution in the amount of serum-albumin (by which he means plasma—hypalbuminosis)."

If we go on we shall find that anæmia or oligæmia vera really means a diminution of the whole quantity of the blood, and that this condition can be subdivided into hydræmia, oligocythæmia, oligæmia sicca, and oligæmia hypalbuminosa. We have, then, three representative definitions: the one broad and general, including a diminution of any one, or all the constituents of the blood; a second, in which hypalbuminosis and oligocythæmia are the principal, if not the only factors; and a third, which makes oligocythæmia alone the characteristic feature. It seems to me that, from a purely practical standpoint, the latter is the best; we are not in a position to determine the whole quantity of blood in a living animal, nor, for more reasons than one, can we make a chemical analysis of blood in any individual case. But we can readily determine the number of red corpuscles in any patient, and, if it be admitted that hypalbuminosis and oligocythæmia go together in the great majority of cases, it will become unnecessary to make a chemical examination of the blood.

A search amongst the authorities for a definition of chlorosis puts us in a much sadder plight than did anæmia. One type of writers is, perhaps, best represented by Coupland in his excellent course of Gulstonian Lectures (*Lancet*, 1881), who says: "I believe chlorosis to be a form of anæmia depending upon similar changes, and, therefore, to be considered as falling under the head of idiopathic anæmia." It is but just to add that this author defines anæmia as that condition in which the essential feature is the reduction of the hæmoglobin. Zender (Virchow's *Archiv*, 1881) conceives chlorosis to consist in defective formation of red blood-corpuscles, both quantitatively as well as qualitatively, and adds, further on, that, as iron is diminished in the blood and not excreted, the fault must lie in defective absorption. Then we have authors like Strümpell (*loc. cit.*), who, defining anæmia as oligocythæmia, state that chlorosis is a term applied to the mild forms of essential anæmia, such as are most often seen in females at the time of puberty. Immermann makes the disease dependent upon a diminution of hæmoglobin, but is not willing to commit himself by stating whether this is absolute for a single red corpuscle or for the whole quantity of blood.

We now come to what seems to me to be the turning-point in the finding of a sharp, distinctive definition, which is to be found in Duncan (*Sitzungsberichte d. Acad. d. Wissenschaften*, Wien, 1867). As far as I know, he was the first to make the distinctive feature oligochromæmia characteristic for

[1] Read before the Association of American Physicians, September, 1889.

chlorosis—that is, in chlorosis we find the individual red corpuscle deficient in hæmoglobin. It will be readily seen that a deficiency in hæmoglobin would exist in any condition in which the number of red corpuscles is diminished; this would be called anæmia according to the definition we have accepted. But the red corpuscles being normal as to quantity and the hæmoglobin diminished, we would be justified in concluding that each individual red corpuscle carries too little hæmoglobin, and this condition would be called chlorosis.

Unfortunately for this definition, the term chlorosis is to be looked upon as an essentially clinical one; it is not going too far to state, I think, that by far the great majority of reported cases of chlorosis are not chlorosis at all. In Germany every other girl is called *bleichsüchtig* (chlorotic), the diagnosis being made by the people and then verified by the physicians; with us chlorosis is a comparatively rare affection, and yet no one doubts for a moment that there are just as many, if not more, anæmic girls in this country than in Germany. In Russia almost all women are said to be chlorotic, especially in the cities; even the lower strata of society are convinced that they have chlorosis. Five or six years ago a great tide of emigration swept from that country to ours; I had occasion to examine a great many of these women, and every case was one of anæmia and not of chlorosis. Within the last year I have seen an American girl who had been compelled to stay in St. Petersburg for several years; she was pronounced chlorotic there by an eminent physician; examination of her blood revealed oligocythæmia (about seventy per cent.) and a proportionate oligochromæmia. This girl was very anæmic, but, according to the conception which we would ask to be accepted, she was not chlorotic.

Another difficulty in the drawing of strict lines consists in the fact that both oligocythæmia and oligochromæmia may occur in the same individual, so that the term chloranæmia is one that is justified by observation of existing conditions. It is more than probable that anæmia is a forerunner of chlorosis in a great many instances, and, possibly, a very great number of cases occur in which there is a combination of both conditions.

As far as the causes for both conditions are concerned, it will be seen that almost every one put down for anæmia will hold good for chlorosis. Without entering into detail upon this division of the subject, it can be stated that sex, age, a peculiar composition of the blood, and certain vascular anomalies first pointed out by Virchow in 1870, are held to be especially characteristic for chlorosis. If we analyze each one of these causes, it will be seen that the latter two are the only ones to be found in chlorosis. While there can be no possible doubt that chlorosis is a disease of females at puberty, yet cases have been described in which chlorosis has been found in males, and a great many cases are upon record in which the disease has developed either earlier or later than puberty (as late as fifty-seven years, Cantrel). Here, again, the "elasticity" of the term chlorosis must be taken into consideration. In regard to chlorosis in males, if we read the report of the case given by Immermann (p. 532, *loc. cit.*), it will be seen, that this author has given us no positive proof at all that his case is not one of anæmia, although he warns against the too ready acceptance of cases of anæmia as chlorosis. The fact that this case was relieved by energetic chalybeate treatment is apparently looked upon by the author as convincing evidence that it was one of chlorosis. In cases of so-called school-anæmia we are accustomed to expect good results from removal from school and the administration of iron.

As to the age of patients, the process of sexual development has always been looked to as the time of most common occurrence of chlorosis; in anæmia this age also produces a great number of cases, but children and old people are subject to anæmia and, practically, excluded from chlorosis. For the explanation of this fact it became necessary to conceive of the existence of some physical anomaly in the sexual organs; lack of development, malformation, etc. The clinical fact that in some cases we find amenorrhœa, in some comparatively normal menstruation, and in others menorrhagia, ought to have been sufficient testimony to show that there was no constant genital factor, and, in some cases, none at all. Investigations upon the post-mortem table have verified these statements, and we, therefore, find the genitals well and normally developed in some cases, in others too great a development, and in others a deficiency. These changes are so varied as to the external genitals, the uterus, and the ovaries, that no causative relation can be established between these lesions and the disease. It is more than probable that the relation is that of effect to cause and not of cause to effect; in other words, that the lesions in the genital organs are due to chlorosis (or anæmia).

It will be seen by examining statistics that by far the greatest number of cases occur between the ages of fifteen and twenty-five—*i. e.*, decidedly after the time of the age of first menstruation, which is supposed to occur between the ages of twelve and fifteen years. Furthermore, the observation that an anæmic constitution is a very strong predisposing cause for chlorosis, has been made by almost every author; some authors, in their description, make cases of anæmia more intense and then call the condition chlorosis. It seems justifiable to conclude that a great many cases of chlorosis have their inception in ordinary cases of anæmia. The continued effort of menstruation and, possibly, the un-

accustomed loss of blood going on for a period of time extending over from three to ten years, in an anæmic patient having the peculiar metabolism of anæmia, seems to be sufficient to establish the condition we consider characteristic for chlorosis. Again, the fact must not be lost sight of that a comparatively slight cause may set up the condition of chlorosis in a female who has always been considered perfectly healthy. Yet, occurrences like these must be looked upon as exceptions.

The clinical characteristic of chlorosis lies in the peculiar changes in the blood—changes which have been denied by some authors, and the strict meaning of which has been accepted by few. In anæmia we have the red corpuscles as well as the albumins of the plasma reduced in quantity; frequently there is a change in size, shape, and quality of the corpuscles (Osler, Cartwright Lectures, 1886). The change in size of the red corpuscle is in the direction of production of a greater number of smaller corpuscles (microcytes). As a result of the reduction in the number of red corpuscles the hæmoglobin is correspondingly reduced in quantity. In pure chlorosis the number of red corpuscles is not diminished at all, or so slightly that the diminution may be ascribed to the method of enumeration. On the other hand, the tendency to the production of red corpuscles of larger diameter than normal (megalocytes) is especially well marked in chlorosis. While we do not fail to find an appreciable number of microcytes, the large number of megalocytes found is apparent upon very superficial observation. Malassez gives the average size of the red corpuscle in chlorosis as 8.29 μ (7.60 being normal), and the relative size as 1.09 μ (1 being normal). These changes, although present to a greater or less extent in every case of chlorosis, are by no means sufficiently characteristic to establish a diagnosis or to enable us to exclude other forms of alterations in the blood. The red corpuscle in chlorotic blood seems decidedly pale to one who has had the color of the individual corpuscle firmly impressed upon his mind. When we deal with large quantities of blood this is apparent to the untrained without any further remark, but the change under the microscope is just as appreciable when the comparison is made with healthy blood. When the blood is examined with the spectroscope or the hæmoglobinometer, the reason for this paleness is readily explained; we find a great diminution in hæmoglobin. But this is found in any form of anæmia, so that it becomes necessary to establish the amount of hæmoglobin in each individual corpuscle. In order to do this we must employ the fraction first suggested by Gowers, in which the numerator is the percentage of hæmoglobin, and the denominator the percentage of corpuscles. It is not necessary, before this Association, to point out the possibilities of errors in this computation. Gowers says of his instrument that it "is only expected to yield approximate results, accurate within two or three per cent." The more recent instruments are more accurate, possibly, but nevertheless errors are unavoidable.

It seems to me that one great source of error which has been overlooked in all these computations is the neglect of taking the average size of the red corpuscle into consideration. It is true that this is comparatively small when compared with an error of three per cent., yet it would be appreciable when we are dealing with a sample of blood in which the average size of the red corpuscle is very much increased. In order to correct this, the following computation could be made, A representing the average size of the red corpuscle of the examined blood, and B Gowers's fraction:

$$A : 7.50 :: B : x, \quad \text{where } x = \frac{7.50 \times B}{A}.$$

Thus, if we take a sample of blood in which, by Gowers's equation, the quantity of hæmoglobin in an individual corpuscle is estimated at $\frac{30}{60}$, and correct by the above formula, using Malassez's figure (8.29 μ), it will be found that the individual corpuscle contains only $\frac{27}{60}$ of hæmoglobin, certainly a decided difference. At the end, however, the point to be made in the establishment of the diagnosis of chlorosis is that the single red corpuscle contains less hæmoglobin than normal. It must be added that the amount must be greatly reduced, because of the fact that our methods of computation are not such as could be called mathematically precise, though sufficiently so for clinical purposes.

The difference in the quantity of albumins may be considered another distinctive feature between anæmia and chlorosis. It is said that in anæmia we always have hypalbuminosis, while in chlorosis this is absent. It must be confessed that the statements of authors are so contradictory upon this question, that a definite conclusion cannot be arrived at. It is necessary that this part of the subject be reinvestigated and then, possibly, a positive result will follow. The statement that chlorosis is a cytogenic affection and anæmia both a cytogenic as well as a general affection of the blood is one that, as yet, cannot be considered proven.

One respect in which chlorosis is said to differ materially, if not absolutely, from anæmia, consists in the pathological changes first described by Virchow. These changes can be briefly characterized as hyperplasia of the vascular apparatus—narrowing of the lumen of the aorta and larger arteries, as well as thinning of their walls. The heart itself is sometimes found small, at other times hypertrophied. According to this view, every case of chlorosis is congenital and must be due to some faulty develop-

ment of the mesoblast; it would permit us to state that chlorosis is always congenital, in some cases latent for years, while anæmia, in most cases, is acquired.

A great many objections have been raised against this view; they cannot "be considered as underlying every case of chlorosis or, at any rate, existing as a cause of the anæmia;" "no proof has been given that the diminution in size of the vessel has not followed upon diminution in the total quantity of blood" (Coupland, *loc. cit.*). The first of Coupland's objections certainly holds good; hyperplasia occurs in a great many males, in a far greater number than will be admitted by even the most enthusiastic defenders of male chlorosis, and, in proportion to the number of females attacked, it is found in a very small number of post-mortem examinations. The second objection—*i. e.*, that we may be dealing with an effect and not a cause—is one that is not easily proven; if it is an effect, it can certainly not be due to a "diminution of the total quantity of blood," for, when this does occur in chlorosis, it is not as great as it is in anæmia, and, probably, with this hyperplasia there frequently occurs a plethora. However this may be, to the best of my knowledge no author has been found who is willing to state that this condition of the bloodvessels is found in any other disease than chlorosis. Even if we are not disposed to look upon these changes as a direct cause, we must certainly be prepared to consider them as predisposing to chlorosis on account of the frequent coincidences of the two.

The relation of the symptoms of anæmia to chlorosis depends upon the conditions of the blood and the anatomical substrata. Given a case of anæmia in which there is only a reduction of hæmoglobin as a result of oligocythæmia, and a case of chlorosis in which there is a reduction of hæmoglobin as a result of oligochromæmia, and metabolism in both of these cases will be approximately the same. It is true that the patient with chlorosis will have a different facies; as the essence of the affection lies in oligochromæmia, the amount of reduction in hæmoglobin, if due to oligocythæmia simply, would be so great as to become a physical impossibility, inconsistent with life. But if, in anæmia, we have oligocythæmia as well as hypalbuminosis, which is said to be coexistent in the majority of cases, the metabolism must be different from that of a pure case of oligochromæmia. As a result of reduction in hæmoglobin we have simply a reduction in the process of oxidation in the body; at the same time the waste products are carried off and sufficient albuminous food is carried to the tissues by the plasma of the blood which is unchanged. In this way the general nutrition of the body is apparently not interfered with. Indeed, there may be, and there frequently is, an increased deposit of material in chlorosis, because oxidation is not sufficient to utilize the albumins of the plasma; the result of this process of suboxidation is a deposit of fat, all of which Immermann calls the "nucleus" of the clinical differences between anæmia and chlorosis.

Hypalbuminosis and oligocythæmia produce an entirely different result; as before, we have the same factor, suboxidation, but we have, in addition, a deficiency in supply. Furthermore, those functions of the plasma otherwise than nutritive are seriously interfered with. Its physical conditions are changed, it does not serve the processes of secretion and excretion as it should, and, being full of excrementitious material, the result is the retention of these substances, the production of fever, etc. In mild cases of anæmia there is produced a loss in weight, in severe cases there is produced that condition which we call marasmus. In chlorosis very little, if any, loss of weight occurs; very frequently the opposite is brought about by a too great supply, and especially by the presence of too much CO_2, preventing the decomposition of the fats. But when anæmia is associated with chlorosis we have a very unfortunate combination, especially where there is a great amount of hypalbuminosis. These are the cases in which the development of pernicious anæmia as a direct consequence of chlorosis can be readily explained. The chlorosis produces oligocythæmia and hypalbuminosis as a result of deficient external food-supply; these two produce the metabolic combination stated above; and, finally, we have established a *circulus vitiosus* into which it seems impossible to break. But these combinations must certainly be looked upon as of extreme rarity.

Unless we believe Virchow's anatomical lesions to belong only to chlorosis, there exists no difference between the symptoms of the two diseases. When these are present they manifest themselves by the ordinary objective signs of whatever change there may be in the heart. We have a normal heart, one that is too small, or one that is larger than normal. The most interesting change that we find is the dilatation of the left ventricle. Considering the hæmic bruits that occur in the heart, it sometimes becomes quite difficult to state whether or not we are dealing with a valvular lesion. The dilatation of the left ventricle may be transitory in its nature, depending, as it does, for its existence upon the quantity of blood to be pumped through the narrowed arteries.

Reference must also be made to symptoms on the part of the nervous system; these have been made one of the characteristics for chlorosis. Hysteria and hysterical conditions are of such common occurrence nowadays, that it seems impossible to make any one condition responsible for them. A careful analysis of female anæmic cases would, I think, demonstrate the fact that just as many of these are

hysterical as in the same number of chlorotics. Before making a careful examination of the blood, it would be very risky to make the diagnosis of oligochromæmia in a patient who is both anæmic and hysterical.

Lastly, we have to consider the therapeutic relations of both diseases. While the reasoning of *ex juvantibus* is certainly justifiable under certain circumstances, it seems to me to be strained in its application by Immermann. He speaks of specific medication, and says that the ease with which iron controls the symptoms of chlorosis is "characteristic" for the latter disease—as "a fortunate relation, which is not found, to the same extent, in any other form of anæmic process, and which, therefore, deserves to be considered as one of the essential peculiarities of the trouble." The degree of success and the time in which it follows the use of chalybeates are to be distinctive for chlorosis as opposed to all other anæmic processes. From a scientific standpoint, this certainly seems a very weak argument to offer, and one that depends, for its verification, upon the individual employing it. To what extent must improvement follow, and in what time, before one case can be called chlorosis and another anæmia? The fact must not be lost sight of, that a great many cases of anæmia are just as much improved by iron as are cases of chlorosis. Although the indiscriminate use of iron in anæmia must be deprecated, yet, upon the whole, iron is just as much looked upon as a specific in anæmia as in chlorosis. As we are led to expect from the essential symptoms of chlorosis, iron would be indicated in every case, and very few authors will be found who do not confirm this therapeutic indication. While, then, we would not be willing to separate anæmia from chlorosis, we are justified in the statement that iron ought to be given in every case of chlorosis, on account of the rapidity and intensity of the chalybeate effect.

Where, in anæmia, we try to remove the cause and institute causal treatment, we are satisfied, in chlorosis, with removing that symptom which is the essential of the disease, either on account of the fact that we are ignorant of the first cause, or on account of our inability to remove it. It has been repeatedly demonstrated that this can be done, and, furthermore, that the removal of this essential symptom is followed temporarily by complete recovery. Whereas, in anæmia, treatment is followed by complete recovery or complete failure, iron in chlorosis will always result in amelioration, even if we cannot remove the tendency to relapses. There are very few cases of chlorosis, even those with the lesions of Virchow, that are not benefited by the administration of iron in sufficient quantity. In a great many cases of anæmia the use of iron would be followed either by negative or by bad results.

AGAINST SECTARIANISM IN MEDICINE.[1]

By EDWARD JACKSON, M.D.,
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The idea on which the following argument is based has been thus expressed by the late Prof. George B. Wood: "Conscientious convictions should be respected, even though based on ignorance and delusion, and so prone is the human intellect to every kind of aberration that we may readily admit the possibility of an honest conversion from orthodoxy in medicine to the wildest creed that ever sprang from a deluded imagination."[2]

There are many things to engender mutual distrust among practitioners of medicine, and set them against one another. Beside the rivalry, which often seems to preclude the possibility of kindly relations in every walk of life, there is much that is peculiar to our profession. The dogmatic way in which a professional opinion must be stated to make it clearly intelligent to the patient, is often at variance with the meagreness of the scientific facts that could be adduced to support it, and we must often speak positively of matters concerning which there can reasonably be great doubt; the demands of the sick-room for positive action are so opposed to the nice balancing of scientific probabilities—that the management of each case lays the physician open to controversial attack. Then, too, in the domain of medicine, so much is half-knowledge and surmise, there is so much room for honest difference of opinion, that uniformity cannot be looked for, while the issues that hang on the correctness of opinion are so important as necessarily to elicit strong feeling, making diversity of view particularly liable to end in strife.

But there are most weighty reasons why strife should be prevented. There is no attitude of mind more unsuited to the perception of truth, the observation and correct interpretation of phenomena, the exercise of the best judgment, than the controversial. The medical controversies of all times illustrate the evils of intolerance, the benefits of coöperation. Whether we study the struggle of the empirics and methodics, of Cullen and Brown, of Rush and most of his prominent contemporaries, or the controversies over the tubercle bacillus, or the treatment of pyosalpinx in our own societies, the lesson is the same. All experience teaches that the condition of professional harmony and coöperation which renders the skill and learning of each available for the aid and instruction of all, furnishes the most efficient service to the community, and conduces most to the welfare, influence, and honor of the profession.

[1] Read before the Philadelphia County Medical Society, September 25, 1889.

[2] Introductory Lectures and Addresses.

To associate all members of the medical profession in hearty good feeling and earnest coöperation has been the dream, the desire, the high purpose of our best leaders in professional organization. Such was the purpose that founded the American Medical Association, shaped its code of ethics, and drew to its support the best part of the profession. That purpose is fulfilled in so far as our organization is made to include every worthy member of the profession, and is defeated in so far as any are excluded. If, as a branch of the American Medical Association, we should stand for any one peculiar idea, it is for that of the organization of the whole profession into one body, as opposed to that of its organization upon lines of medical dogma into separate and antagonistic sects.

In the process of organization it became necessary to indicate who should, and who should not, be regarded as members of the profession. But the only exclusive clause in our code of ethics reads: "No one can be considered a regular practitioner or fit associate in consultation, whose practice is based on an exclusive dogma, to the rejection of the accumulated experience of the profession, and of the aids actually furnished by anatomy, physiology, pathology, and organic chemistry." The same paragraph of the code says: "A regular medical education furnishes the only presumptive evidence of professional abilities and acquirements," but gives no definition of a regular medical education other than such as may be inferred from the foregoing passage, viz., that it shall not be confined to the study of a single dogma, or set of dogmas, or neglect the aids of anatomy, physiology, or organic chemistry.

There is, however, a tendency to narrow the interpretation of the word "regular" until it shall no longer convey its proper meaning, namely, that the person to whom it is applied has successfully completed a full course of medical study; but merely that he belongs to a certain school or sect in medicine, which differs from other schools or sects mainly as to the importance of certain medical dogmas or doctrines.

As generalities are so easily misunderstood or misinterpreted, let us take a special case. In this city there is an institution called "The Hahnemann Medical College and Hospital." I certainly do not share the feeling that led to the naming of it from Hahnemann. Still it is conceivable that, in spite of its name, the study of anatomy, physiology, or organic chemistry will yield within its walls very much the same results as elsewhere; and the only fair way to judge whether or not its graduates have received a regular medical education is to examine its curriculum, and to find out how thoroughly that curriculum is enforced.

In judging how the compilers of our code of ethics used the phrase "a regular medical education," it should be borne in mind that at the time the code was drawn up there was no homœopathic medical college in existence. An institution to teach homœopathy had been started a few years before at Allentown, Pa. It soon became extinct. But it had conferred the degree, not of Doctor of Medicine, but of Doctor of Homœopathia, and its curriculum could not be fairly said to offer a regular course of medical instruction. At this time, too, the homœopathic medical societies consisted often largely of laymen, and a large proportion of those who were professing to practise homœopathy were as destitute of any medical education as many of the faith-curers of to-day. Such being their lack of qualification for medical practice, they, of course, fell under ban as irregular practitioners. But time that changes all things has wrought a transformation in the homœopathist that many of us have not fully realized. Consider the requirements for graduation in the Hahnemann Medical College, as set forth in its last annual announcement, and I know of no reason for doubting that these requirements are lived up to as well as are the corresponding requirements in most of the medical schools that we all agree are "regular." The course is a three years' graded course, the length of term six months. In anatomy, two courses, four lectures a week, and the text-books are Gray's and Wilson's. In practical anatomy, one course, one lecture a week; text-books, Holden's and Allen's. In physiology, two courses of lectures, three a week; text-books, Dalton and Foster. In general pathology, two courses, one lecture a week, with the use of Green's text-book. In chemistry, two courses, three lectures a week, and the text-books of Draper, Ganot, Fownes, and Attfield, with a course in practical chemistry, including laboratory work. The student is recommended to study: in surgery, the works of Bryant, Erichsen, Holmes, Gross, Agnew, and Smith; in obstetrics, those of Playfair and Lusk. Even for practice of medicine he is sent to Loomis, Roberts, or Pepper's *System;* for therapeutics, to Stillé and Maisch's *Dispensatory;* for pharmacy and toxicology, to the *U. S. Pharmacopœia*, Mann on *Prescription-writing*, Taylor on *Poisons*, Wood's *Materia Medica*. But the list need not be further extended. Suffice it to say that of all the text-books this institution recommends to its students, about two-thirds are of authorship unquestionably orthodox, while the remainder are largely compilations from sources equally creditable. Can it be claimed that those so educated have based their practice "on an exclusive dogma, to the rejection of the accumulated experience of the profession, and of the aids actually furnished by anatomy, physiology, pathology, and organic chemistry?"

For them to pretend that they practise an exclu-

sive system of medicine is an imposture. But for us, by our attitude toward them, to support that pretence is imbecility. We do support it if we exclude them *èn masse*, giving as our reason their system of practice.

But when it is admitted that they are practitioners who have received a regular medical education, it is claimed that they should be ostracized for false pretence. Let those who are guilty of false pretence feel such disapprobation. But the penalty may be made most effective by confining it strictly to those who have deserved it; and the guilt or innocence is to be determined separately for each individual case. I believe those connected with the institution I have referred to, do not place upon sign, or card, the words homœopathist, homœopathic doctor, or other words of similar import, and that they discourage such a practice. The announcement from which I have quoted bears the motto: "*In certis unitas, in dubiis libertas, in omnibus charitas.*" The most prominent adherents of the doctrine of "like cures like," publicly deny that their practice is based exclusively upon it. Not long ago several of them resigned their positions in one of their hospitals in this city, rather than practise it exclusively in an institution founded for its illustration and propagation. The societies into which these practitioners have banded themselves make no profession of an exclusive dogma. Some, as the American Institution of Homœopathy, require no expression of belief whatever.

Much of what has been said of this group which is commonly regarded as made up of irregular practitioners may, with slight modification, be said of another, the so-called eclectics. If I can understand the profession made by both of these sects, it is that they take experience as their teacher and guide. If the experience they follow leads them in certain directions to absurd conclusions, looking back upon the history of medicine I cannot therein find ground for their exclusion from all professional recognition. "Conscientious convictions should be respected."

The medical profession can never afford either to prescribe or proscribe opinions for its members. Whenever you divide medical practitioners upon a line of dogma, whether that line be to include the believers in a certain doctrine, or to exclude them, you introduce with all its evils sectarianism. Let us hope that the American Medical Association and its code of ethics, designed to bind together the profession into one harmonious whole, shall not, by false interpretations and failure to understand its catholic spirit, be suffered to hinder such a union.

Women's Medical College of Cincinnati.—Dr. George A. Fackler, for a number of years Assistant to the Chair of Materia Medica at the Medical College of Ohio, has been elected Professor of Materia Medica in the Women's Medical College of Cincinnati.

AN ETHICAL QUESTION.[1]

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It is true that no man is so sure of the accuracy of his knowledge or the wisdom of his judgment, that he can refuse to respect the honesty with which others maintain opinions from which he may dissent. Nevertheless there are certain facts of nature which have passed beyond the region of opinions into that of established truths: there are certain methods which the well-settled judgment of scientific men requires to be pursued in our interrogation of nature for yet other truths. To respect the honesty with which a man entertains beliefs contradicted by all that is known of natural law, and by the collective experience of mankind, is one thing: to dignify manifest absurdities with the name of scientific opinion, is quite another.

The Rev. John Jasper and the Grand Llama of the Trans-Baikal are honest enough in their opinion that the sun makes a daily circuit about the earth; but scientific men and the world of thought they influence, have long since accepted as final the contrary teaching of Copernicus and Galileo, and will not now intermit more important labors to debate the question.

But though we may respect the man who honestly errs, we are not called upon to entertain any such feeling toward him who is ignorant through wilfulness; and as for him who, knowing truth, wears for hire the livery of error, even divine charity can ask us to do no more than withhold judgment.

I have no desire to attack the opinions or the characters of any body of men—certainly not those of the homœopathic practitioners of this city, many of whom I have the pleasure of knowing as cultivated and honorable gentlemen. When, however, the charge of illiberality is made against the members of this Society and of the profession of medicine, because they refuse to recognize certain practitioners as entitled to the honored name of physician, it becomes proper, in repelling that charge, to exhibit plainly the reasons which make such recognition impossible.

The question interjected by Dr. Jackson into the deliberations of this Society, when stripped of the sophistry with which it has been overlaid, is a very simple one. Disregarding the attempts to erect fine distinctions upon unsubstantial grounds, the gist of the matter may be tersely and bluntly stated as follows: Can physicians conscientiously consult with, or recognize as members of their profession, prac-

[1] Read, as a reply to Dr. E. Jackson's paper on "Against Sectarianism in Medicine," before the Philadelphia County Medical Society, September 25, 1889.

titioners of that system of therapeutics known to the public, in the language of its founder, as "homœopathy"?

Proceeding to discuss this as a casuistical question, there are two hypotheses to be taken up in turn, upon each of which an independent answer must be given.

The first hypothesis is, that all who are called homœopathists are what they profess to be, and allow the public to consider them; namely, practitioners of homœopathy: homœopathy being the system of therapeutics invented by Hahnemann, and asserted by its founder (*Organon*, § 53) to be based upon an "eternal and unerring law of nature," expressed in the formula "*similia similibus curantur*" (or *curentur*). Upon this hypothesis I shall show by citations from the most authentic and authoritative exposition of homœopathic doctrine and practice, namely, Hahnemann's *Organon*, that consultation between physicians and homœopathists could be productive of no result; for there is no possible common meeting-ground. It is not a mere question of the expediency of this or that therapeutic measure in a given case, such as often comes up in discussion between physicians, but one of fundamental difference in principle, making agreement as to practice impossible—if both be honest. Their doctrines and their practices, whichever be right and whichever be wrong—a question which it is needless to discuss—are mutually antagonistic and destructive. From this point of view alone, and leaving out of consideration the larger ethical questions of public duty, it is obvious that a consultation would be condemned from the first to be a barren union, and must therefore be an immoral one. The question as to its permissibility will thus be at once answered in the negative; and it has so been answered by sincere homœopathists.

The second hypothesis is, that men who profess to be homœopathists, who allow the public to consider them such, and who accept the fees of persons desiring homœopathic treatment, are not what they profess to be, but something else. It matters not what that something else is; so long as it is not consistent with homœopathy, it is something that contradicts the profession made to the public.

Upon this hypothesis—which I do not allege represents the existing state of affairs, but which, if I understand Dr. Jackson aright, he distinctly declares to be the case so far as concerns the Managers of the Hahnemann Medical College of Philadelphia—I shall contend that there is a very important question of public honor and private honesty involved; and that until it can be shown that it is right to take money or other valuable considerations under false pretences, no honorable man can consult with practitioners of this type—if any such practitioners exist—for he would, by so doing, become accessory to a fraud. Let me repeat, in order not to be misunderstood, that I do not assert that anybody whatever is guilty of the false pretence of professing to be a homœopathist while in reality he is something else. I shall show by citations from the gospel of homœopathy—namely, the *Organon* of Hahnemann—what homœopathy is. Then, if the state of affairs described by Dr. Jackson is actual, and he is not laboring under a misapprehension as to the facts, it will become self-evident that the practitioners whom he describes have been refused the name of homœopathists by the one who had the best right to define that name;[1] and the question as to the propriety of consulting with them will again be answered in the negative.

My propositions, then, are three:

1. Either the so-called "new-school physicians" are what they profess to be—homœopathists—or they are not.

2. As to the honorable men who are what they profess to be: We cannot honestly consult with them, nor do they wish to consult with us—because consultation would be fruitless.

3. As to the not-honorable men who are not what they profess to be: We cannot honestly consult with them, because we would become participants in an act of deception.

This is the issue as I understand it, freed from glossing and concealing verbiage. Truth requires it to be frankly stated and as frankly met.

To prove these propositions I must establish two things:

1. That the practitioners in question profess to be homœopathists, or "new-school physicians."

2. That the principles and practice of homœopathy, as set forth by competent authority, are such as to be utterly irreconcilable with the principles and practice of medicine and with the sciences upon which medicine is founded, as these are understood by the great body of modern physicians and by the members of this Society.

The first of the facts I must establish, I did not, until to-night, understand to be denied by any one. It has seemed to me as much a matter of public knowledge, as the fact that President Harrison professes to be a member of the political party called Republican, and that ex-President Cleveland professes to be a member of the political party called Democratic; that the Archbishop of Canterbury is a dignitary of the Anglican Church, and the Cardinal at Baltimore, of the Roman Catholic Church. For

[1] See a paper by Edward Bayard, a homœopathist by conviction and not "for revenue only," entitled "Plea for Pure Homeopathy against Eclectic Homeopathy." Proceedings in the Homœopathic Medical Society of the County of New York, on March 8 and 13, 1878.

the sake of completeness of proof, however, let me cite these facts: There is in existence a "Homœopathic County Medical Society" in Philadelphia, and a "Homœopathic State Medical Association" in Pennsylvania. Members of these bodies succeeded in defeating the efforts of this Society, and of the Medical Society of the State of Pennsylvania, to secure the passage of an act establishing a State Board of Examiners and Licensers, by alleging the existence of separate and irreconcilable "schools of medicine," and thus securing the adoption of an amendment providing that the majority of the proposed Board should not be adherents of any one "school." Not only does the Hahnemann Medical College connect itself by its name with the author of the *Organon of Homœopathic Medicine*, in whose mystic, metaphysical brain the strange delusion originated; but it distinctly recognizes as its associates other homœopathic colleges, and accords their students and graduates such privileges as colleges of medicine accord to students and graduates of colleges recognized by them. Furthermore, I have never heard, nor do I think Dr. Jackson has ever heard, of cases or fees having been refused by the professors or graduates of the Hahnemann Medical College on the ground that patients desired the services of homœopathists, while said professors and graduates were not homœopathists. Nor has there been any public announcement that the Hahnemann Medical College is not a homœopathic college, or that its professors or graduates have realized the errors of homœopathic ways and have repented and abandoned them.

If, then, the case of Dr. Jackson rests upon the fact that the Faculty of the Hahnemann Medical College and its graduates are not professedly homœopathists, I respectfully submit that the very parties concerned would not endorse his special plea, and it consequently falls to the ground without further waste of words. Until the teachers and practitioners in question publicly repudiate homœopathy, I cannot see why we should assume that they have clandestinely done so. And until they do repudiate homœopathy, as I shall show in a moment, a Surgeon-General's Library of scientific text-books could not neutralize the absurdities to which its profession commits them.

I have devoted to this subject much more time than it deserves, in order to prevent misunderstanding of the position. I confess, that to carry on my argument I must establish the fact that the profession of homœopathy is made by certain persons. I claim that I have established that fact by citing certain matters of public knowledge. Dr. Jackson must either admit that I have established this point, or he must frankly meet it and show that these persons neither actively profess to be homœopathists nor tacitly allow their patients and the public so to consider them.

Secondly, I must show that homœopathy and scientific medicine are absolutely irreconcilable; and I must show this upon unimpeachable authority.

It is neither my purpose to defend those against whom the inventor of homœopathy directed his invective and his sarcasm, nor to detract from his possible[1] services to medicine in attacking some palpable errors of his time. Nor do I wish to debate the question of the truth or error of the homœopathic doctrine. To waste time in demonstrating before this body the fallacies and gratuitous assumptions upon which that doctrine is founded, would be as superfluous as it is needless to my argument. For whether the so-called "law of homœopathic cure" be a profound truth or an insane delusion, all that I need show is its incompatibility with what we recognize as sound teaching concerning the nature of disease, the action of remedial agents, and the manner in which the physician should proceed in investigating the one and applying the other.

If the authority to which I shall refer for every step in this proof is not beyond cavil, then, indeed, may we at once admit that the moon is made of green cheese, that two and two never did make four, and that words have no meaning. It is the sacred scripture of the great prophet of the "new-school of medicine"—Samuel Hahnemann's *Organon of Homœopathic Medicine*, in an authorized translation,[2] edited by the prophet's greatest disciple and apostle in America, C. Hering, and certified by the "Chairman of the Board of Directors" of the "Academy of the Homœopathic Healing Art," at Allentown, Pa.

If I am not in error, the *Organon* of Hahnemann still holds an honored place as a text-book at the Hahnemann Medical College of Philadelphia, as it does at the homœopathic colleges of other cities.

Time necessitates limitation of the number of quotations,[3] and the selection of the most characteristic. It will be impossible to cite *verbatim* the full text of every section to which reference may be made; but wherever I have condensed the loose-jointed and bewildering verbiage, I have scrupulously endeavored to be fair and correct.

From the *Organon* of Hahnemann I purpose to show four things, any one of which is fatal to the possibility of fruitful consultation.

1. Homœopathy as expounded by Hahnemann,

[1] Possible, but exceedingly problematical; inasmuch as he inveighed indiscriminately against error and truth, bad practice and good practice; and the substitute he offered has been proved worthless.

[2] New York, Wm. Radde, 1849.

[3] For the same reason no reference has been made to the excellent expositions of the fallacies of homœopathy by various writers.

its inventor, is exclusively a system of drug-therapeutics; explicitly renouncing the aid of the *vis medicatrix naturæ*;[1] and denouncing the study of anatomy, physiology, pathology, etiology, semeiology, nosology, and diagnosis.

2. It is an arbitrary and exclusive system; inflexible; and claiming to be infallible.

3. It expressly prohibits the endeavor to interpret morbid symptoms as indications of changes in the structure or functional activity of definite tissues and organs; declares that an all-wise Providence has denied to man the power of learning what takes place within the human body; with logical consistency rejects the attempt to direct treatment against definite pathological conditions and processes or their dangerous consequences; and fixes attention upon the symptoms alone, independent of their origin, as constituting the whole of the disease, and affording the sole indication for treatment.

4. The doctrine of *homœopathia*—or the casting out of "natural diseases"[2] by artificial diseases closely resembling them, but excited by drugs—is inextricably bound up with the practice of infinitesimal dosage.

If these points be established, the logical inference will be not only the utter impracticability of consultation between physicians and genuine homœopathists; but also that those who make diagnoses—direct treatment to the production of definite effects upon particular tissues or organs—give any other than "homœopathic" remedies—refrain from the use of drugs in any case—or give drugs in any other than infinitesimal dosage—are not true homœopathists. If any such profess to be homœopathists, we will be compelled to conclude that they are either deliberate deceivers or, at best—in the language of Hahnemann—members of "the new mongrel sect" who try to "find a pretext everywhere for . . . exceptions to the general rule, in order to introduce their allœopathic palliatives, with other mischief of a like character, merely to save themselves the trouble of searching for suitable homœopathic remedies for every case of disease—one would say, to save themselves the trouble of being homœopathic physicians, though they wish to be considered such. But," continues the prophet, whose hearty contempt for such insincerity provokes admiration, "their deeds will follow them; they are of little moment."[3]

I. Homœopathy is exclusively a system of drug-therapeutics.

The introduction to the *Organon* of Hahnemann consists principally of denunciations of all medical practice prior to, or differing from that of, the author; and especially of the efforts of physicians to discover the causes of disease, in order to direct treatment, where possible, to the removal of those causes. Nothing, according to Hahnemann, is more vain and irrational than to endeavor to seek out the cause of disease. "It was the boast of the former schools of medicine,"[1] he says, "that their doctrine alone deserves the title of 'rational art of healing,' because it was pretended that they alone sought after and removed the morbid cause and followed the traces of nature herself." A vain boast, because "by far the greater number of diseases [are] of dynamic origin as well as of a dynamic nature, and their cause therefore [does] not [admit] of discovery to the senses." Among the irrational procedures committed in this foolish search after causes, he especially condemns the habit of "comparing, on the one hand, the normal state of the parts of the dead human body (anatomy) with the visible changes which those parts had undergone in subjects that had died of disease (pathological anatomy), and on the other, the functions of the living human body (physiology) with the endless aberrations to which they are subject in the various stages of disease (semeiotics, pathology), and drawing from thence conclusions relative to the invisible manner in which the changes are brought about in the interior of man when in a diseased state."[2]

Thus if—together with etiology—anatomy, physiology, pathological anatomy, semeiology, and pathology by name, and diagnosis by implication, are denounced as senseless and misleading, there is little left of medical science; and of its art, simply applied therapeutics. That this is the most important to the patient is true; and it is the goal to which our studies of normal and morbid anatomy, of physiology, pathology, and other sciences, are to lead us; but surely none here present will subscribe to the doctrine that the goal can be reached by any other road than that which the inventor of homœopathy so explicitly rejects and denounces.

One or two additional citations will serve to show more clearly the significance for our second main hypothesis, of this position of Hahnemann's. Meaning to be satirical, he says of physicians: "The sublime project of discovering . . . some internal and invisible cause of disease resolved itself . . . into a search, guided onward by the symptoms, after that which they might term to be the generic character of the existing malady. They endeavored to find out whether it was spasm, debility, or paralysis, fever or inflammation, induration or obstruction in

[1] *E. g.*, op. cit., pp. 40, 42, *et seq.*

[2] The two diseases fight it out, and the patient escapes.

[3] Note to § 67, ed. cit., p. 132.

[1] Ed. cit., pp. 26 and 27.

[2] The context, which I have omitted, denounces, it is true, some particular conclusion said to be thus reached; but our concern being only with the explicit condemnation of the method, it is of no moment whether the conclusion be or be not truthfully stated or justly criticised.

some one of the parts; excess of blood (plethora) or increase or deficiency of oxygen, carbon, hydrogen, or nitrogen in the fluids; exaltation or depression of vitality in the arterial, venous, or capillary system; a defect of relative proportion in the *factors* of sensibility, of irritability, or of nutrition."[1]

Allowing for the animus of the author, for changes in terminology, and for the progress of science, this would describe with a sufficient degree of accuracy the course still pursued by the intelligent physician in the endeavor to reach a diagnosis. To Hahnemann, however, such a course was fit subject for ridicule, and in a foot-note he says: "Every physician adopting a treatment of such general character"—that is to say, every physician who seeks to make a diagnosis, or to arrive at a knowledge of the morbid changes in the organism which he is called upon to combat—"however unblushingly he may affect to be a homœopathist, is and always will remain a generalizing allœopathist, as without the most special individualization *homœopathia* has no meaning." Surely no language can be plainer than this!

This denunciation of diagnosis and nosology, following, as it does, the explicit rejection of anatomy, physiology, and the other branches of medical science, and showing the complete dependence of the whole homœopathic system upon a deliberate shutting out of the light, becomes readily understood upon looking further into the peculiar theories of Hahnemann. He assumes the existence of an "immaterial vital principle pervading the physical organism" (§ 11). "It is solely the morbidly affected vital principle which brings forth diseases[2] (§ 12)." "The vital principle as a spiritual dynamis" (whatever that may be) "cannot otherwise be assailed and affected than in a spiritual (dynamic) manner; neither can such morbid disturbances be removed by the physician, except in like manner by means of the spiritual (dynamic virtual) countervailing agency of the suitable medicines acting upon the same vital principle" (§ 16).

Here we have not only the affirmation of the spiritual nature of disease, of its causes and of its cures—explaining the futility of the rejected studies which concern themselves with the material body, and its material morbid changes—but also the distinct assertion that diseases can only be removed by medicines; while again and again, throughout the introduction, and in a mass of foot-notes to various sections of the *Organon*, are violent denunciations of the idea of trusting in any degree whatever to the efforts of nature, or of accepting as a guide to therapy indications derived from a study of the natural history of disease. Let me cite one of the mildest of these maniacal outbursts: "The miserable and very imperfect attempts which the vital powers make to assist themselves in acute diseases, is a spectacle that ought to excite man to use all the resources of his learning and wisdom, to put an end, by a real cure, to this torment which nature herself inflicts. If nature cannot cure, homœopathically, a disease already existing in the system, by the production of a fresh malady *similar* to it (§§ 43–46)—a thing not often in her power to effect (§ 50) . . . we see nothing but painful and often dangerous efforts of the constitution to save itself at all hazards, efforts of which death is most frequently the result."[1]

I have thus, I believe, established the proposition that homœopathy, as expounded by Hahnemann, is exclusively a system of drug-therapeutics.

II. It is an exclusive system, arbitrarily devised, inflexible, and claiming infallibility.

"Observation, reflection, and experience have unfolded to me," says Hahnemann, "that in opposition to the old allœopathic method, the best and true method of cure is founded on the principle *similia similibus curantur*. To cure in a mild, prompt, safe, and durable manner it is necessary to choose in each case a medicine that will excite an affection similar to that against which it is employed. Until the present time no person has ever inculcated this homœopathic mode of treatment, and yet more, no one has ever put it into practice. . . . In all ages the diseases which have been cured by medicines . . . have yielded, although without the knowledge of the physician, to a homœopathic remedy . . . a remedy in itself capable of exciting a morbid state similar to that whose removal it effected."[2]

Having assumed that for every disease there must be a remedy—and that this remedy is a medicine—he further assumes that medicines can accomplish the cure of disease, only in one of three ways: (1st) by exciting a disease similar to the natural disease, which he calls "homœopathy;" or, (2d) by exciting a disease opposite to the natural disease, which he calls "antipathy;" or (3d) by exciting a disease which is neither similar nor opposite to the natural disease; and this, which he incorrectly states is the common practice of physicians, he dubs "allœopathy" (§ 53). Antipathy and allœopathy have been tried up to his time and have failed; consequently, homœopathy must be the only true method (§§ 24, 61, *et al.*).

The "discovery" of the homœopathic "law" was not the result of experience, but, according to his letter to Hufel,[3] was reached by reflection,

[1] Ed. cit., p. 28.

[2] Compare also this gem from the introduction (ed. cit., p. 35): "Diseases will not, out of deference to our stupidity, cease to be *dynamic aberrations which our spiritual existence undergoes in its mode of feeling and acting.*"

[1] Ed. cit., p. 42.

[2] Ed. cit., pp. 59, 60.

[3] Cited by Henderson: Homœopathy Fairly Represented. Phila., 1854. Pp. 116 et seq.

before any experiment had been made. "God," he says in effect, "made diseases; God must also have made a remedy for each disease; and He indicates its use by giving it power to produce in healthy persons symptoms like those of the disease it is intended to cure."

I have used the expression "exciting disease" in referring to Hahnemann's exposition of drug action. It is his own language. In many cases it is direct, as in § 29, where he says in conjectural explanation of the homœpathic cure, "The natural morbific power terminates . . . while the *medicinal disease*, which usurps its place, being easily subdued by the vital power, is . . . extinguished in its turn." But it is also implied throughout when he speaks of the symptoms produced by drugs, because it is one of his fundamental principles that, so far as human observation is concerned, symptoms and disease are one and the same (§§ 11–21).

Our way is now clear to set against one another certain fundamental propositions of Hahnemann's: "Every curable disease is made known to the physician by its symptoms" (§ 14). "The physician has only to remove the totality of the symptoms and he has cured the entire disease" (§ 17). "The totality of the symptoms is the sole indication in the choice of the remedy" (§ 18). "The morbid symptoms which medicines produce in healthy persons are the sole indications of their curative virtues in disease" (§ 21). "The homœopathic method, or that which employs medicines producing symptoms similar to those of the malady, is the only one of which experience proves the certain efficacy" (§ 24).

"It is as impossible that there should be any other true method of curing dynamic diseases . . besides homœopathy, as that more than one straight line can be described between two given points" (note to § 109). "By this method alone can we cure diseases in the most speedy, certain, and permanent manner, because it is grounded upon an eternal and unerring law of nature" (§ 53).

These citations should be sufficient to establish the second thesis—namely, that homœopathy is an exclusive system, that it is arbitrarily devised, that it is inflexible, and that it claims to be infallible. "An eternal and unerring law of nature" can admit of no exception and no failure. Upon the surface of the earth, at the centre of the sun, or at the extremest limit of the orbit of Neptune, gravitation acts with a force proportional to mass and inversely as the square of distance. In the tropics, as in the arctic regions, like poles of magnets repel and unlike poles attract. The atomic weight of carbon, the valence of hydrogen, differ not in the brain of man, or in the fossil leaf. These are matters governed by "eternal and unerring laws of nature." If the "law of homœopathic cure" is entitled to be called a law, it must be equally infallible and exclusive to-day, as when Hahnemann penned his sweeping claim. Maintenance of that claim precludes any modification in principle or practice; "More than one straight line *cannot* be described between two given points." Abandonment of the claim is abandonment of homœopathy.[1]

III. Homœopathy forbids diagnosis[2] and condemns the attempt to direct treatment to definite ends, etc.

It follows from what has been quoted, and is repeatedly, explicitly, and elaborately stated by Hahnemann, that the sole function of the homœopathic practitioner, after having inquired minutely into the symptoms of the case before him, is to select from among the various medicines known to him, that one the "disease-producing power" of which most closely corresponds to the "natural disease" (§§ 83 et seq., 146 et seq.). This is the sole and infallible method (§ 109). If additional evidence is required on this head, the following should suffice:

"There is nothing for the physician to cure in disease but the sufferings of his patient, and the changes in his state of health which are perceptible to the senses—that is to say, the totality or mass of the symptoms by which the disease points out the remedy it stands in need of; every internal cause that could be attributed to it, every occult character that man might be tempted to bestow, are nothing more than so many silly dreams and vain imaginings" (§ 70). "Just as little as we can witness what is passing in the interior of our bodies in a healthy condition—and as certainly these processes remain concealed from us as they lie open to the sight of Omniscience—just so little can we perceive the internal operations of the animal frame, when life is disturbed by disease."[3]

"They" (physicians) "likewise suppose that by removing a polypus by ligature, extirpating a tumefied gland . . . dissecting out the insulated cyst of a steatomatous or meliceretous tumor, operating for aneurism, fistula lachrymalis, or fistula in ano, amputating a cancerous breast . . . etc., to have cured the maladies in a radical manner and destroyed their cause. . . . *But mark what follows.* New forms of disease . . . infallibly manifest themselves sooner or later . . . being always worse than the primitive affection. . . .

[1] See Declaration of Homœopathic Principles; with Comments by Adolph Lippe, dated Philadelphia, Feb. 4, 1878; and Protest against a certain Resolution adopted by the Homœopathic Medical Society of the County of New York; signed by C. Lippe, E. Carleton, Jr., S. Baruch, et al. Reprinted from *The Organon*, Liverpool, April, 1878. The resolution, *which was afterward rescinded*, declared that belief in the principle *similia similibus curantur* "does not debar us from recognizing and making use of the results of any experience;" and it is protested against, because, "*not being in accordance with the teachings of Hahnemann, [it] cannot be accepted by Homœopaths.*"

[2] See especially § 100.

[3] Introduction, foot-note, ed. cit., p. 42.

These ought to undeceive them and prove that the evil has an immaterial cause, the deeper concealed because its origin is dynamic, and it cannot be destroyed but by dynamic power."[1]

If such obvious procedures as tying an artery for aneurism, dissecting out a sebaceous cyst, or amputating a cancerous breast, are thus condemned, what shall we think of such impious attempts to pry into the processes going on in the interior of our bodies —"as surely concealed from us as they are open to the sight of Omniscience"—as are involved in the diagnosis of consolidation of a lung, stenosis of a cardiac orifice, or tumor of the brain? And what of measures that disregard symptoms *in toto* and endeavor to attack the pathological process underlying them?

IV. The doctrine of "homœopathia" is inextricably bound up with the practice of infinitesimal dosage.

"The appropriation of a medicine to any given case of disease does not depend solely upon the circumstance of its being perfectly homœopathic, but also upon the minute quantity of the dose in which it is administered. If too strong a dose be given, it will infallibly injure the patient. The impression it makes is felt more sensibly because in virtue of its homœopathic character the remedy acts precisely on those parts of the organism which have already been most exposed to the attacks of the natural disease" (§ 275). "A strong dose of homœopathic medicine would do more harm than the dose of an allœopathic medicine of equal strength; for in that case the homœopathic aggravation—that is to say, the artificial malady excited by the remedy in the most suffering parts of the organism—is carried to a height that is injurious" (§ 276). This, of course, we should naturally imagine; that in so far as the effects of medicines do at all resemble the symptoms of diseases, the result of giving such medicines would be highly injurious. Instead of being taught by this the folly of his methods, Hahnemann, being, like Ephraim, wedded to his idols, takes refuge in a new absurdity, and concocts the doctrine of infinitesimals.

In order to determine just how far homœopathic medicines would have to be attenuated in order to be rid of their power for evil, Hahnemann had recourse to experience; and he found it necessary so to dilute the dose that it amounts to practically nothing. The well-known process is to dissolve two drops of medicine in ninety-eight of alcohol, one drop of this in ninety-nine more of alcohol, and so on until the necessary minuteness is attained, at from the tenth to the thirtieth dilution:[2] provided care be taken not to shake the containing vessels more than twice at each successive dilution, as otherwise too much "dynamic spirit" would be freed; and ten shakes might even make the medicine so powerful as to be extremely dangerous.

These directions and cautions are found in the *Organon*, beginning with Section 269, and elaborated in the succeeding twenty sections.

It is possible, however, for even the minutest dilution of homœopathic medicine to be too strong for a very sick patient. "In that case" we find[1] that "it will be sufficient to let him smell it once." In the process of smelling,[2] the patient should place a vial containing one globule of sugar the size of a mustard-seed, impregnated with a sufficiently attenuated dilution of the desired remedy, under one nostril, and take one momentary inhalation. If the dose is intended to be stronger, he may then repeat with the other nostril. The operation of the remedy continues as long as when the same drug is taken in substance, and hence the dose must not be repeated at shorter intervals. In Section 247, we are told that "the most subtile doses of the best chosen homœopathic medicines" are to be repeated at intervals of fourteen, twelve, eight, to seven days; except in acute cases, when the periods may be abridged to twenty-four, eight, or four hours, and in the most acute cases down to five minutes.

In a case of pneumonia, I suppose, the patient might safely smell the medicine once in four hours; in a case of mitral stenosis, once in a week;[3] and in a case of chronic gastric catarrh—not at all.

The citation of this nonsense is not unfair quibbling upon my part. It is the essential part of the true homœopathic practice. Any fair-minded person who reads what Hahnemann says upon this subject, his frequent strong, repeated affirmations that "the more homœopathic the medicine, the greater danger" if it be given in any but the most attenuated doses; his denunciations of the "pretended homœopathists" who give larger doses than he directs; his elaborate and minute directions as to the manner of preparation; forbidding powders, for example, to be triturated "with force"[4] for more than one out of the three hours which he considers necessary to effect liberation of the spiritual dynamic essence;—any fair-minded man, reading all this, cannot but admit that the doctrine of similars and the doctrine of infinitesimals must stand or fall together.

And that he is painfully in earnest as to the smelling business, is evidenced by the following statement

[1] Ed. cit., p. 33.

[2] Representing one grain of a medicinal substance dissolved in sufficient alcohol "to make one hundred and forty billions spherical masses extending from limit to limit of Neptune's orbit, or a quantity equal to many hundred spheres each with a radius extending from the earth to the nearest fixed star."—*Simpson.*

[1] Note to § 285. [2] Page 209.

[3] "Let the patient once strongly smell a globule as large as a mustard-seed impregnated with *hydrarg. met.* X., which is to be allowed to operate about nine days."—Page 208.

[4] Page 225.

—a statement which I certainly do not care to dispute: "All that is curable by homœopathy may with most certainty and safety be cured by this mode of receiving the medicine."[1]

Does it need comment of mine to show how utterly impossible it is for a physician to meet in consultation one who gives the slightest credence to the rot which I have quoted?[2] The physician's first course, as I have had the pleasure of observing in cases in which I have solicited aid from the special knowledge of my friend Dr. Jackson, is to make a diagnosis; the homœopathist is bound to reject diagnosis. The physician seeks out the causes of disease, that he may remove them, if possible; that he may intelligently combat their effects; that he may prevent disease in the future. The homœopathist is committed to the doctrine that disease consists only of symptoms whose cause has been concealed by an all-wise Providence. The physician may use or omit drugs; the homœopathist is bound to employ drugs, though, it is true, in such infinitesimal doses that if needless they will at least be harmless. But this negative virtue of the attenuated medication vanishes in cases in which drugs are imperatively needed, for then the homœopathist is precluded from employing them in effective quantity: because, if homœopathic, they will infallibly do harm; and, if not homœopathic, his system declares them to be murderous.

But it may be said that the homœopathist is no longer bound by the *Organon* of Hahnemann; he is open to the lessons of experience, and homœopathy is no longer to him an infallible law of nature, but an occasional guide in practice.

This brings us face to face with an exceedingly unpleasant portion of our task, that of considering our second hypothesis; namely, that men who now profess to be homœopathists are in reality something else. However, as we are merely considering it as a hypothetical, casuistical question, we may at least avoid direct personal reference.

Can one remove the foundation and expect the building to stand? Considering the origin of homœopathy, its claims to universal and infallible application, the explicit declaration of its founder that no other system could be true, even as more than one straight line could not be described between two given points; it must be much more than an occasional guide in practice, or it must be what scientific men regard it, a baseless fabric of assumptions. As has been well said by Prof. Gairdner, the vigorous imagination which can attribute nine hundred and thirty symptoms to charcoal, and ten hundred and ninety to oyster-shell, can find little difficulty in fitting a sufficient number of the confused mass of "provings" belonging to any drug, to any case in which recovery may have taken place coincidently with its use. Mere experience, then, need not take any out of the homœopathic ranks, if he be not possessed of a scientific conscience whose crying refuses to be quieted with sugar plums.

But a homœopathist cannot deliberately reject Hahnemann's *Organon* and remain entitled to the name. Hahnemann invented the name, or at least gave it its peculiar significance; Hahnemann has defined it, and no one has the right to alter Hahnemann's definition. He put forth homœopathy as a new system in medicine, differing *in toto* from all other systems, rejecting alike the methods of investigation and the methods of treatment pursued by the great body of physicians. As he formed it, so it must remain; for he claimed the authority of a divine revelation,[1] the infallibility of "an eternal and unerring law of nature."

If the barrenness of its history during the years that have been so fruitful for medical science; if the brilliant results of that patient search after material morbid agencies so derided by Hahnemann; if the triumphs of preventive medicine, in which a system founded upon drug-giving could have, and has had, no part; if the progress of physiological therapeutics, which having learned how remedies act, now joins forces with chemistry to attack the why:—if these things unite with continued experience in the sick-room, to prove the wretched inadequacy of Hahnemann's system; so that even in the temple builded to his name strange fires are burning upon the altar, so that even among his priests and votaries the light of the fires of science is breaking through and dispelling the gloom of his mystic and obscuring speculations:—if intelligent men, entering the homœopathic ranks through prejudice or force of associations, and in ignorance of the facts, have learned by study and experience the hollowness of the faith to which they profess allegiance—then there is for these men but one honorable course—the public acknowledgment of the facts. It may be unpleasant. It requires moral courage. But it is right, and it will compel respect.

But so long as a name having commercial value

[1] Note to Section 288, p. 226. It is a cheap method, too. He states that one globule will retain its virtues undiminished for twenty years, though it had been smelled during that period a thousand times. Let alienists tell us how much dependence can be placed upon the observation of a man who seriously makes such assertions.

[2] And I have by no means cited the most ridiculous passages of the *Organon*. I have said nothing of the "psoric miasm," for this is not essential to homœopathy; and, indeed, like the exceptions Hahnemann himself was compelled to make concerning "surgical diseases" and the "*causa occasionalis*," logically contradicts it.

[1] . . . "The certainty of medicine depends on the establishment of fundamental principles, based on natural laws, to guide the Healer in his Therapeutics; and . . . Samuel Hahnemann *was allowed to reveal to the world* such principles and their application for the cure of the sick, in his *Organon of the Healing Art* . . ." Declaration of Homœopathic Principles, loc. cit.

as a trademark is retained by any body of men, so long do the barest principles of trade-honor bind them to all the implications of that name. If they can reconcile it with their consciences to continue to bear a name to which they have lost the right, we at least dare not impute to them, without sufficient evidence, conduct so base and cowardly; or, if we have such evidence, then we must not, by condoning duplicity, become partakers in dishonor.

CLINICAL MEMORANDA.

HYGIENIC.

Possibility of Establishing Tolerance for the Tubercle Bacillus.—During the last summer I spent much time in bacteriological laboratories abroad, studying the different methods of manipulation and technique. The first culture given me to stain by Prof. Klein, at the College of State Medicine, London, contained tubercle bacilli. The tube had been kept in an incubator used by all the students in the College, not only for legitimate purposes, but for warming of staining materials, and, therefore, it goes without saying, that the temperature was not at all constant. The first slides prepared from this culture showed club-shaped bacilli. Suspecting a lower degree of virulent life, I permitted the tube to remain at the temperature of the room for several days, before staining a second series of slides. Upon these I found many bacilli in the forms represented in the accompanying cut.

When Prof. Klein's attention was called to the newly discovered forms of bacilli, he expressed much surprise, and was unable to account for the change in their forms, and yet was quite certain that the tube in question was inoculated with bovine tubercle bacilli after the method generally employed. From this tube I made a series of tube inoculations, and grew bacilli which in every respect corresponded to the ordinary tubercle bacillus. From the facts already given, I have seen fit to institute a series of experiments, the results of which will be published from time to time, and which lead to the following thoughts.

In considering a means for overcoming infection by tuberculosis it is probable that a condition of tolerance to the action of the tubercular bacillus must first be established. To this end two hypotheses may be suggested:

First. It is possible that, by a thorough filtering out of bacilli from tubercular material, a filtrate might be obtained and attenuated so that by systematic inoculations a change might be produced in living animal tissues that would enable them to resist virulent tubercle bacilli.

Second. To bring about a chemical or physical change in living tissues that would resist tubercular phthisis, it is possible that inoculations with the bacillus would have to be made; yet, before this could be done, the power of the virulent bacilli would have to be diminished, otherwise the result would be most disastrous.

The accompanying cut represents tubercle bacilli in other forms than those heretofore recognized, and it is possible that they represent the condition of bacilli necessary to prove the truth of the second hypothesis, particularly as animals inoculated with these organisms have survived subsequent inoculations with virulent tubercle bacilli.

Samuel G. Dixon, M.D.,
Professor of Hygiene, University of Penna.

MEDICAL PROGRESS.

Death from Chloroform.—Another case of death during the administration of chloroform is reported in the *British Medical Journal* of October 5, 1889. A lady, about twenty-five years of age, of a very nervous, excitable temperament, desired to have some teeth extracted, and insisted upon an anæsthetic. Her medical attendant administered chloroform. The patient was seated in an easy chair, and, after inhaling a few breaths of chloroform, she slipped down in the chair, and her pulse and breathing were both found to have stopped. Artificial respiration was at once resorted to, but without success. It seemed perfectly clear that the patient was not anæsthetized when she died, as she had only just commenced to inhale the chloroform, and, of course, no attempt had been made to extract her teeth. The coroner's jury returned a verdict that death was due to syncope, and that no blame whatever was to be attached to the administrator.

Syncope, as has long been known, will result from any very violent emotion, and especially from the effects of fear. In a case recorded in Germany a few years back, a female patient visited a dentist, and requested him to extract some carious teeth, demanding, at the same time, that she should be chloroformed. The dentist, very properly, explained the risks of chloroform, and suggested nitrous oxide; but his patient persisted, and he consented to humor her. Having, however, a wholesome dread of chloroform, he substituted eau-de-cologne, and bade her inhale the supposed anæsthetic from a folded towel. After two or three inspirations she suddenly fell from the chair, and died. That death occurs from fear in some cases during the earliest stage of chloroforming is unquestionable; and as it is predisposed to by the sitting posture, and by forcibly restraining the patient's voluntary movements, there can be very little doubt that in every case in which chloroform is to be administered the recumbent posture should be insisted upon, and a loose dressing-gown substituted for the usual every-day costume. There is another point of no small importance, which is, that chloroform increases the liability to death from "reflex syncope." A person partly under chloroform is more prone to die from fright than one to whom chloroform has not been administered. Nor must it be forgotten that deaths occur when only one or two inspirations of chloroform have been taken; this is liable to take place when a too-concentrated vapor (that is, one of greater strength than four per cent.) is employed. In the event of syncope occurring in one of the ways above indicated, the line of treatment which offers the greatest chance of success is

total inversion of the patient, while care is taken that the rima glottidis is maintained open for the entrance of air. Dr. Chisolm, who has strongly supported this, Nélaton's original manœuvre, has recorded some highly instructive cases in which the method of inversion, being promptly performed, effected resuscitation, and so saved the patient's life. But here a caution must be given. Nélaton's method is valuable only in primary syncope, and is absolutely dangerous in cases of heart failure consecutive upon pulmonary engorgement and overfilling of the right heart; or, in short, when respiration stops before the heart ceases to beat. Cases belonging to this last category do not occur, as a rule, until the later stages of chloroformization, and the respiratory failure is then due to overdosage with the anæsthetic.

Functions of the Stomach in Phthisis.—Simultaneously with the recent investigations regarding the prophylaxis of phthisis, the accompanying symptoms and various forms of this disease have been most actively discussed, both here and abroad. Of these, phthisical dyspepsia is perhaps the most common and most unpleasant symptom. German investigators have dealt with the subject most extensively.

DR. G. KLEMPERER,[1] after an examination of a large number of cases, places them in two categories—initial and terminal dyspepsias. The initial dyspepsia is characterized by heart-burn, eructations, nausea, a feeling of fulness of the stomach, and not infrequently vomiting, which is usually excited by violent coughing, and is independent of the ingestion of food. The appetite is variable, and varies from rejection of food to insatiable hunger. Klemperer believes dyspepsia to be frequently the first symptom of early phthisis. Terminal dyspepsia is characterized by constant loss of appetite; severe vomiting usually occurring every day or two, and the stomach frequently becoming greatly distended. Primarily, the trouble is merely a weakness of the muscles, but finally there is almost total cessation of the secretions.

Therapeutically, Klemperer especially recommends remedies which tend to stimulate the motor power of the organ. Foremost among such drugs is creasote, and he has found that alcohol and bitter tonics are also valuable.

Another writer on this same subject, DR. O. BRIEGER, of Breslau,[2] has examined sixty-four phthisical patients in various stages of the disease with the following results: In severe phthisis only 16 per cent. of the cases had a normal condition of the stomach, in the rest a more or less complete gastric insufficiency was noted. In 9.6 per cent. an entire loss of the normal secretive power was observable. In cases where the disease was moderately severe 33 per cent. were found to be free from gastric complications, and in 6.6 per cent. an entire absence of the normal secretions was observed.

Brieger lays great stress upon the value of "overfeeding," or increased alimentation, if the stomach will permit it.

DR. C. HILDEBRAND,[3] in examining the gastric secretions of forty cases of phthisis, found free muriatic acid in some patients, while in others it was absent. He further noted that in the former fever was absent, whilst in the latter it was constant. In a third group of cases, in which the fever was intermittent, it was found that the presence and quantity of hydrochloric acid depended largely upon the condition of the temperature. Hildebrand, as well as Brieger, has obtained excellent results with forced alimentation.

[1] Berliner klin. Wochenschrift, No. 13, 1889.
[2] Deutsche med. Wochenschrift, No. 14, 1889.
[3] Centralblatt für klin. Med., September 21, 1889.

Laparotomy for Hydatid of the Liver.—DR. DAVIES THOMAS, in a paper on echinococcus cysts (*The British Medical Journal*, September 28, 1889), says that it is not possible to estimate the value of tapping, parasiticide injections, and electrolysis, because the cure which often appears to follow is frequently illusory. He sees reason to believe that tapping operations fail to cure in fully 40 per cent., and probably in a larger proportion. Taking aspiratory punctures and ordinary tapping operations together, the deaths amounted to nearly 18 per cent.; but the mortality following aspiratory puncture, whether single or multiple, was only about half that of punctures with an ordinary fine trocar. Dr. Thomas believes that the greater the number of punctures required the smaller the probability of cure, and points out that, though simple puncture is generally devoid of risk, it has been known to cause sudden death, sometimes apparently from shock—sometimes, however, in the case of pulmonary hydatids, from suffocation by the fluid contents of the bladder-worm. The objection to puncture as the mode of treatment for internal hydatids, however, lies less in the occasional perils of the operation than in its frequent inefficacy.

Dr. Thomas considers that nothing can be said in favor of parasiticide injections into the sac and the employment of electrolysis that does not apply with equal force to simple puncture; and, moreover, each possesses drawbacks of its own. The statistics of abdominal section for hydatid of the liver compiled by Dr. Thomas are extraordinarily good. He has collected sixty-eight cases thus treated, with only seven deaths; in one of these seven cases the patient died suddenly during the operation from sudden arrest of the heart due to a hydatid cyst in it; in two others there were multiple hydatids in the body. Even including these three cases, the recoveries amounted to within a fraction of 90 per cent. The method by two stages, producing peritoneal adhesion by incision and packing with carbolized gauze (Volkmann's method) showed a mortality of a fraction over 19 per cent.; the operation by caustics gave a mortality of 33.68 per cent., while that by the *canule à demeure* was 26.66. The cases treated by Volkmann's method only numbered twenty-one, but even so the statistical evidence appears to be strongly in favor of abdominal section as the safer procedure. Where unruptured cysts are present on the convexity of the liver, the treatment offers greater difficulty, for Dr. Thomas states that thoracic incisions show a high rate of mortality. As to pulmonary hydatids, however, the risks and disadvantages of puncture are so great and the results of radical operations on the whole so favorable, that Dr. Thomas considers that there ought to be but little difficulty in making a choice of treatment in ordinary cases. Where a hydatid, whether of the liver or of the lung, has ruptured in the pleura, free incision into the pleura appears to be the only treatment which holds out a fair promise of success.

Medical Treatment of Ileus.—Among European investigators none, perhaps, have so carefully studied the therapeusis of ileus as DR. NOTHNAGEL. After an experience of many years he now bases the medical treatment of this disease on the following principles: Excitation of peristalsis by means of irrigation, and salt water enemata or ice-water injections, opium internally to guard against fatal collapse, entire abstinence from all food, and exclusion of all cathartics. Irrigation of the stomach, although practised by many, is of no particular value; still, as it is harmless, it may be tried, and may give the patient some ease. Massage is emphatically contra-indicated, as is also the use of electricity.

When given early in the disease Nothnagel favors the use of mercury. His experiments upon animals have proven the fact that the drug never causes reversed peristalsis, either in the normal bowel or when stenosis exists.

He asserts that the vomiting of fecal matter is caused mechanically. When an effort is made to vomit, the abdominal muscles are forcibly contracted, and the fecal mass, being compressed, seeks the easiest outlet, which is above.

A Novel Vermifuge.—While the following advice seems to us as apt to kill the host as the guest, we quote it for the benefit of our readers from the *Journal of Pharmacy* for September:

A whole cocoanut, grated fine, mixed with milk, and taken on an empty stomach on rising, is fully as reliable a tæniafuge as male fern, kousso, or pomegranate, etc.; it is far more agreeable to the taste. No after-treatment is necessary, as a single dose usually is all-sufficient.

A New Turpentine Eruption,—DR. ERNST FEIBER, of Aix-la-Chapelle, in the *British Journal of Dermatology*, furnishes an excellent clinical example of the very rare and unique condition of an œdematous and papular erythema following the internal administration of a turpentine preparation. The patient was under treatment for subacute gonorrhœa, and was taking ten minims of the oil of turpentine twice daily. On the ninth day of the treatment—that is, after the use of nearly three fluid-drachms of the turpentine preparation—the patient complained of severe pruritus. On the following day an erythema, consisting of small, acutely pointed papules of a bright red color, some of which were excoriated by scratching, was found to cover the entire surface of the body excepting the head and face, being more pronounced on the lower than on the upper limbs, and specially intensified on the abdomen and buttocks. The pruritus and rash disappeared in three days, on the cessation of the turpentine preparation. Being desirous of ascertaining if the turpentine would again produce the erythema, Dr. Feiber again prescribed it. On the fourth day violent itching returned, accompanied by an eruption of similar papules; but in addition to these there were noticed on the abdomen and buttocks small red, slightly raised spots, with patches about the size of a florin of the same bright red color, both in their centres and in their peripheries. The urine was normal, and the patient felt well. Dr. Feiber remarks that whereas the first eruption was exclusively a pure papular erythema—roseola, the second, on the contrary, was a mixed form of œdematous and papular erythema, an eruption described now, as far as he knows, for the first time as consequent on the administration of turpentine.—*The Practitioner*, September, 1889.

Quiet Gout.—This term, says MR. JONATHAN HUTCHINSON, in the *Archives of Surgery*, may be suitably employed as applicable to the many cases in which those who are gouty experience no material inconvenience. The phenomena by which this condition is recognized are infinitely varied, and some of them very peculiar. A great many of them are probably caused by locally restricted attacks of peripheral neuritis. He regards the following as a case in point: A gentleman, past middle age, who had never suffered from an attack of gout, but who for many years had experienced many warnings, became the subject of a very real but almost ludicrous affection of one knee. He could walk quite well, could bear to have the knee handled any way, and there was not the slightest swelling. There was one thing, and only one, which he could not do without pain: he could not kneel on any soft substance. If he took care how the knee was placed on a hard floor, he could bear the pressure without pain for some time, but to kneel on a cushion might produce an explosion of pain which would make him cry out. He was often, so to speak, "caught" when in the act of getting into bed. The pain was of the nature of a local explosion, and was limited to an area the size of a crown piece. Very curiously, after one explosion had occurred it was difficult to produce a second which should in the least approach it in severity, and, after two or three shocks, he could kneel firmly without any pain whatever. The liability to the attacks passed away under treatment by alkalies and restricted diet, but subsequently returned and lasted a month or two. The skin on the front of the knee became for a time decidedly anæsthetic, a point which seemed to confirm the view that there was a local peripheral neuritis. —*The Practitioner*, September, 1889.

Hydrochlorate of Hyoscin as a Sedative.—DR. MAGNAN, of Paris, who has lately tested hydrochlorate of hyoscin in his hospital, finds that a dose of 1 milligramme ($\frac{1}{64}$ grain) renders the patient calm in five minutes; he appears as if slightly intoxicated by alcohol, his feet seem rooted to the ground and he ceases talking. The drug is valuable in the treatment of insanity, as under its influence lunatics become calm, and remain so. In delirium tremens it induces healthy sleep. A child of thirteen suffering from tic, under its influence was cured of the spasmodic movements. A hysterical patient suffering from profuse perspiration of the hands and arms was entirely relieved by means of an injection of ½ milligramme, the perspiration was arrested in two minutes. —*The British Medical Journal*, September 28, 1889.

Treatment of Eczema, Psoriasis, and Pityriasis.—At the International Congress of Dermatology and Syphilography, DR. GOURBAULT reported a number of cases of eczema, psoriasis, and pityriasis, which he had rapidly cured by the following syrup and ointment: Sixteen ounces of the syrup contain—of bicarbonate and acetate of soda two drachms each, also two drachms of extract of gentian, and a similar quantity of extract of sarsaparilla; a small amount of rhubarb and senna may be

added at the physician's discretion. The dose should be two fluidounces daily.

The formula for the ointment is as follows:

℞.—Ergotine } . . āā ℥ss.
Perchloride of iron }
Vaseline . . . . . ℥j.
M. et fiat unguent.

This should be applied to the affected parts twice a day.

Antipyrin.—One of the greatest dangers which threatens the life of a new drug is the probability of its being looked upon as a panacea. Then, as a rule, after a brief period of notoriety, the drug fades away into obscurity, and even those good qualities which it does possess are looked upon with suspicion. No drug of late years, perhaps, has been so wronged in this way as antipyrin; and when one reads that in New York and elsewhere "antipyrin tippling" has become a common custom, the welfare of the drug seems indeed precarious. Yet antipyrin has held its own so far, and its sphere of usefulness seems to be broadening. The latest investigations with the drug have been probably those of DR. S. CARAVIAS, conducted under the guidance of Professor Germain Sée. He sums up the various actions of the drug in the *Centralblatt für klinische Medizin* for September 28, 1889.

Antipyrin decreases the excitability of the sensitive nerve-centres. Administered cutaneously it greatly alleviates pain of various origins. By means of a direct influence upon the vaso-motor system, it causes an enlargement of the peripheral vessels. Antipyrin in a 50 per cent. solution is a most excellent antiseptic, superior even to a 1 to 1000 solution of sublimate.

Clinical experiments have shown that antipyrin is a most excellent anodyne. It has yielded most satisfactory results in acute articular rheumatism, neuralgias, lancinating pains, etc., and its use, except in a few exceptional cases, has been unattended by any unpleasant accompanying symptoms. Good results were also observed to follow its use in gonorrhœal rheumatism, in dyspeptic troubles, and also in asthma.

The dose employed by Caravias ranged from 47 to 93 grains. In case the internal use of the drug is not tolerated by the stomach, it should be given hypodermatically. This form of administration is also preferable when a quick action is desired.

Regarding the antiseptic properties of antipyrin, Caravias has used a 10 per cent. solution as an injection, and medicated bougies containing twelve grains of the drug with great success in gonorrhœa.

The Treatment of Angioma by Electrolysis.—In the *Centralblatt für Chirurgie* for September 7th, DR. TH. GESSLER gives a report of 221 cases of angioma occurring in the Tübingen clinic. Speaking of the various methods of treatment employed, Gessler records 60 cases treated by excision; 73 by ignipuncture, and the remainder either by the galvano-cautery, ligation of their nourishing arteries, or injection of chloride of iron solution. In 10 of the most severe cases electrolysis was resorted to, and in 9 of these it proved to be successful.

Electrolysis has been rarely used in Germany, but widely employed in England and America. In France its employment has been limited.

The method of treatment employed at the Tübingen clinic was as follows: Two platinum needles were thrust into the base of the angioma, care being taken that they did not touch each other; then a constant current, of about ten to twenty milliampères, was allowed to pass through the growth for ten or fifteen minutes. A burn was caused which healed quickly when treated with iodoform. The procedure must be repeated frequently during a period of one or two months before a complete cure can be effected.

The Cause of Death from Burns.—A series of experiments recently conducted by DR. WELTI, are reported in the *Centralblatt für klinische Medicin* for September 21, 1889. The research was undertaken with a view of ascertaining the cause of death so frequently observed to follow superficial burns of a comparatively slight nature. From an etiological point of view Welti's researches are most interesting.

The first experiments consisted of holding a rabbit's ears in water, the temperature of which was gradually increased up to about 160° to 170° F. The animals all died within an hour. In the other experiments the animals' ears were merely dipped once into water of about 160° F. The majority of these died in one or two days.

In each case the cause of death was manifestly identical, as the same changes were observable in every instance. This change consisted in a degeneration of the red blood-corpuscles, and the formation of thrombi loaded with blood plaques in many vessels, especially those of the pia mater, kidneys, and intestinal tract. When the animals lived for several days after the experiments, pathological results of the above changes were noted. Owing to the obstructions, the different tissues underwent softening and the free surfaces of mucous membranes broke down. Observations in the human subject, under similar circumstances, revealed identical pathological changes.

"Amminol" for the Disinfection of Sewage.—In the issue of THE NEWS for September 14th, a new method of precipitating sewage, then being tested at Wimbledon, was referred to. The *London Times* now devotes a large amount of space to the consideration of this new disinfectant method, which was discovered by MR. WOLLHEIM, of London, and which bids fair to revolutionize the sewage question. The disinfecting power of amminol gas is such that, when introduced into sewage, it very quickly destroys the microbes of putrefaction and of many diseases. The odor of sewage is almost instantly displaced by that of the reagent, and in less than an hour the sewage thus treated is both deodorized and sterilized. It is reported that Dr. Klein has in part confirmed the claims of the discoverer in so far that one sample of sewage examined by him was found to be absolutely sterile after having been treated by the amminol method. If this alleged discovery should be verified, it will undoubtedly become one of the most useful discoveries of the present day and must materially influence the future of sanitary practice.

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SATURDAY, OCTOBER 19, 1889.

ANÆMIA AND ITS TREATMENT.

THE interesting papers recently read by DR. HENRY as referee and DR. FORCHHEIMER as co-referee, before the Association of American Physicians, which have appeared in the columns of this journal within the last few weeks, naturally call attention to the question of the treatment of the maladies which they so fully discuss. Let us first glance at the theories held by some of the various writers on this subject, before turning to the drugs actually to be employed; and in doing this, we must of necessity study to a slight extent the failure of the absorption of iron under ordinary circumstances.

We have the theory of Bunge that sulphur in excess in the body prevents the assimilation of iron, and its endorsement by Clark. If the latter is quoted correctly, his own statement contradicts itself, for he says that in chlorosis sulphides are formed in excess, and this causes chlorosis by non-assimilation of iron. The question at once arises, How did the chlorosis come on in the first place, before the sulphides were formed? Sulphides may aid in its continuance, but it is not proved that they produce it, and it is proved that they are often present in enormous quantities where no chlorosis occurs. Again, we have no proof that the manufacture of hæmoglobin depends upon the absorption of iron alone.

Clark thinks that when we give iron to such a case the sulphides unite with it and the iron in the food escapes into the blood-making organs. Either the doses of iron must be very accurately fitted to the amount of sulphides, or excessive amounts must be used, so that no sulphide may be companionless. As all persons have sulphides present in sufficient amount to make a characteristic stool on taking iron, we should, according to this theory, all have anæmia unless we protected the iron in our food by a constant use of the free metal.

We think practical experience does not support the use of enormous doses of iron in anæmia and chlorosis, but rather the contrary. The celebrated teachers of the Dublin school years ago put forward the theory that small doses of iron were more useful than large ones in direct ratio to the severity of the disease, and we are much inclined to agree with them. There is quite as much likelihood of failure of assimilation of iron by diseased organs as of its destruction by wolf-like sulphides.

We learn from practical experience several things which science does not tell us, namely: Iron will not cure all cases of chlorosis or anæmia, even if they belong to a functional type, but other methods must be resorted to at the same time, or separately, for their cure. The insoluble salts of iron are better remedies for absorption than the soluble salts, because they are not precipitated in the stomach, and for this reason reduced iron is the best chalybeate for uncomplicated cases. Beside using iron in this form, we should employ laxatives if the bowels are confined, mineral acids, particularly nitric and nitro-muriatic, for their effect on the function of the small intestine and liver, and, in addition, bitter tonics of a more or less simple form, according to the exigencies of the case; using quinine if malaria seems lurking in the body, simple bitters if the stomach lacks in tone, aromatic bitters if, from atony of the muscular coats, the digestive tract seems sluggish, with a resulting formation and accumulation of flatus and digestive juices.

At present we may conclude that pathology has not found out the real causes of these troublesome cases, and that until physiology can put its finger upon the most intricate mechanism of blood-making with a positive, clear statement of its function and the physiological chemistry of the manufacturing of hæmoglobin, we must remain satisfied to resort to the remedies which experience or common sense tells us will be of value to our patients.

HOMŒOPATHIC VIEW OF MODERN HOMŒOPATHY.

PERSONS not members of the medical profession, often assert that regular physicians are exceedingly bigoted because so many of them exhibit great feeling when the question of homœopathy is brought forward for discussion. The reason for the bitterness of many medical practitioners is, however, not far to seek. It is because their experience has taught them, that the bulk of the homœopathic practitioners owe, in great part, their success to their not practising what they preach, to their claiming that they avoid strong medicines while at the same time they use drugs in much the same manner as do other doctors.

A recent editorial in *The Journal of Homœopathics* seems worthy of being regarded as witnessing the decadence of modern homœopathy from Hahnemannism. We notice also in the same journal the query whether the American Institute of Homœopathy, having a guilty conscience, was afraid of Judge Barrett's arraignment. It seems to us odd that any legal decision should have been necessary to prove that a man who habitually does exactly what he professes most strenuously not to do is a fraud and a hypocrite, but there are many in whom the discovery, and not the crime itself, produces remorse. The editorial is as follows:

"Perhaps no stronger comparison can be presented between the methods of Hahnemannian Homœopaths and the Eclectic Homœopaths than the recommendation of President Talcott, of the American Institute of Homœopathy, in his annual address at Minnetonka Beach, and the action of the International Hahnemannian Association in 1886, in relation to the same subject, viz., specialties in medicine.

"Specialism in treatment presupposes that there are different means of producing diseased conditions than that which Hahnemann has presented—*i. e.*, something different than a 'distunement of the spirit-like Dynamis (life-force), in the invisible interior, vivifying our bodies. If this supposition is true, then there is no ground for the Institute to stand upon, as an organization separate from the allopathic school; if it is untrue, then the very suggestion places the Institute in the doubly false position as a supporter of homœopathic principles and an opponent of homœopathic practice.

"There is absolutely no place for specialists in homœopathic treatment, except the specialism of *similia*. If there is sickness presenting itself by symptoms in one particular organ or set of organs, it is not because those organs alone are sick, but because the entire individual is sick, and the cure must be effected throughout the entire individual; otherwise there is no *cure*.

"If the American Institute will take pains to study Homœopathy and Hahnemann's *Organon* for the ensuing year, there will be some hope that such absurd blunders will cease in the near future. Such matter, as this we criticise, is, however, but a mere caricature of homœopathy.

"It is only in the International Hahnemannian Association that homœopathy pure and simple is to be found—the Institute abandoned it long ago."

THORACENTESIS.

THE subject of the treatment of collections of fluid in the chest must always be of interest to the practitioner of medicine, because diseases giving rise to such effusions are so constantly met with that every physician must frequently be called upon to treat them.

In *Lyon Médical*, September 8, 1889, Leclerc and Tillier record a case of death from exhaustion following empyema, where thoracentesis had been practised with irrigation of the pleural cavity. One of the chief points of interest in the case is that the injection of a warm antiseptic solution gave rise to the most alarming symptoms, absolute instantaneous arrest of heart action and respiration, with a brachial monoplegia which remained after the more threatening symptoms had been successfully combated. That the symptoms were not due to the antiseptics used is evident from the fact that they appeared immediately, before sufficient time had elapsed for absorption. This case and others illustrate the danger attendant upon irrigation of the pleural cavities, and that such danger should exist is not to be wondered at when we consider the important structures contained in the very mobile mediastinum. That the practice of washing out the emptied pleural cavity is unnecessary, the statistics of Bowditch demonstrate very clearly.

Thoracentesis by itself is not without risk fortunately minimized by the substitution of aspiration for tapping with trocar and canula. Foremost among the dangers stands "albuminoid expectoration," or, to name the lesion instead of the symptom, the overfilling of the capillaries of the lung,

rapidly expanding to occupy the vacuum produced by withdrawal of the exudate. Beside this there is the risk of pulmonary embolism, as well as that of producing convulsive or paralytic phenomena which are usually transitory, but which may, as in the case above mentioned, persist for a longer time.

That there are dangers incident to the operation of thoracentesis is a reason not for discontinuing its use, but for extreme care in its performance. That it should not be unnecessarily performed is as certain as that it often is the means of saving life.

Past experience has taught valuable lessons in regard to the handling of pleuritic effusions. That the fluid when serous can often be speedily removed by medicaments, notably by the use of the "dry method," is undoubted. The withdrawal of even a small portion of the mass of fluid may wonderfully accelerate the complete absorption of all of the exudate. Should the amount of fluid be so large as to threaten life, or should its absorption be found (after a proper trial of non-operative measures) to be slow or imperceptible, the operation of thoracentesis by means of a *small* aspirating needle is indicated to remove either a part or all of the liquid.

Quite different is the aspect of the case when hectic appears, or when exploration with a hypodermatic needle reveals the presence of pus. Here the only means of cure is to evacuate the pus either by aspiration, or far preferably by incision and drainage with or without exsection of portions of one or more ribs. These cases run a far better course if no injections whatever are made, and we believe in the large majority of instances the use of such washes distinctly increases the danger of a fatal issue.

REVIEWS.

A Treatise on the Science and Practice of Midwifery. By W. S. Playfair, M.D., F.R.C.P. New (fifth) American from the seventh English edition; edited, with additions, by Robert P. Harris, M.D. Octavo, 670 pages. Philadelphia: Lea Brothers & Co., 1889.

After the lapse of four years a new edition of this well-known work appears, sufficiently Americanized upon the points where English and American obstetricians differ in opinion and practice, to fit it for the uses of medical students and obstetricians in this country. We find some portions of the book rewritten and several new illustrations added, but by far the greatest change is seen in the additions and notes of Dr. Harris. The pages devoted to the anatomy and physiology of the organs concerned in parturition are practically identical with those of the earlier editions. The precise mode in which the blood escapes from the vessels in producing the menstrual discharge, has been studied by Leopold upon a young girl burned to death on the third day of her menstrual period, and his observations corroborate the view of Coste—namely, that the blood transudes through the coats of the capillaries by a process of diapedesis, while the same observation rather disproves the theory that the entire mucous membrane, or even any portion of it, undergoes the fatty degeneration and extensive exfoliation as are so commonly described. Part II., in which pregnancy is considered, will give the student a clear and concise idea of conception and generation. The author is to be congratulated upon his clear elucidation of the chapter devoted to embryology, a subject which is always so obscure to the average reader. While allusion is made to the employment of abdominal palpation and the fact that it has not been generally appreciated in obstetric works, the author, we think, has not given the subject as much attention as its importance merits. An illustration and less than a page of text describe the method. There is, however, no mention of its value in diagnosticating occipito-posterior presentations, nor the possibility of ascertaining the approximate size and compressibility of the fœtal head. Dr. Herbert R. Spencer, at a recent meeting of the London Obstetrical Society, pointed out the possibility of making the diagnosis of placenta prævia by this method.

In discussing the treatment of extra-uterine pregnancy, prior to rupture, the author states that the uncertainty of diagnosis has prevented the adoption of abdominal section and, so far as electricity is concerned, says, "this practice is perfectly safe and there can be no rational objection to its being tried."

The American editor's note clearly indicates the necessity of employing the exsection method in dealing with the cyst and placenta in cases where the fœtus is living and viable.

About two hundred pages are devoted to labor, in which are ably discussed the phenomena and management of labor, the mechanism and management of normal and faulty presentations, anæsthesia, and dystocia.

The author approves of the intra-uterine applications of styptics as a *dernier ressort* in treating post-partum hemorrhage, but makes no mention of the intra-uterine tampon of iodoform gauze, as first recommended by Dührssen.

In considering obstetric surgery as applied to many causes of dystocia, Dr. Harris, in his editorial notes, has displayed again his intimate knowledge of what has been accomplished by the obstetric surgeon—a knowledge, indeed, so accurate and far-reaching as to merit the highest praise.

It is most gratifying to learn that the Porro operation has fallen in its rate of mortality since 1884 from 58 to less than 20 per cent.; that laparo-elytrotomy has given way to the conservative, or improved Cæsarean, by reason of its diminished death-rate from 45 per cent. to a general average of 20 per cent., and for Continental Europe of 12 per cent.

The chapters describing obstetric operations are admirable, particularly that on Cæsarean section.

The work concludes with ten chapters on the puerperal state, the last one on pelvic cellulitis and pelvic peritonitis, in which the author refers to the importance

of distinguishing the two classes of cases—an importance which recent abdominal work has failed to recognize.

Playfair's Midwifery has for a long time enjoyed a well-deserved place of honor among the standard works on obstetrics, and we see no reason why it should not continue to be one of the best books for the student or practitioner.

WOOD'S MEDICAL AND SURGICAL MONOGRAPHS. Volume I. Number 1. Octavo, pp. 258. New York: William Wood & Co., January, 1889.

The first volume of this series of monographs opens most appropriately with the well-known lectures of Mr. Jonathan Hutchinson, delivered in 1881, on The Pedigree of Disease, which it would be supererogation on our part to review and which are above criticism. A more fitting introductory essay to a series could not well have been selected. Following these six lectures on Temperament, Idiosyncrasy, and Diathesis, are eight on Common Diseases of the Skin, by Dr. Simon, of Birmingham. In them, pruritus as a symptom, eczema, psoriasis, scabies, acne, and ringworm are ably discussed from the standpoint of the general practitioner, with much more attention to treatment than to pathology and dermatological nomenclature. Dr. Simon, in most of these diseases, rightly considers the general management of paramount importance, and devotes six pages to the constitutional treatment of eczema, adding one more protest against the indiscriminate use of arsenic in all but the most chronic cases.

Last and least in practical value are fourteen lectures of Dr. Ferrand's on The Varieties and Treatment of Bronchitis. Classification seems chiefly to have occupied this author, describing, as he does, congestive bronchitis, catarrhal bronchitis, inflammatory bronchitis, spasmodic bronchitis and infectious bronchitis, with nearly fifteen sub-varieties. On the whole, these lectures, though interesting, must often be puzzling to those accustomed to broader views and less minute classification. In treatment there is but little originality, revulsives, inhalations, and nauseant depressants all being advised. Nauseants Dr. Ferrand uses very largely, even when free secretion is established. The volume is printed on the best of paper and in the clearest type.

A GUIDE TO THERAPEUTICS AND MATERIA MEDICA. By ROBERT FARQUHARSON, M.P., M.D., etc. Fourth American from the fourth English edition. Octavo, pp. 598. Philadelphia: Lea Brothers & Co., 1889.

The existence of the classical works of Wood, Ringer, and Bartholow makes the addition of smaller and less complete works appear to us unnecessary. Dr. Farquharson has given us a book fairly up to date in regard to our knowledge of drugs and their effects, but to that knowledge he has added not an iota. The work before us partakes both of the nature of a quiz-compend and a text-book. Used as the former it would be better for the student than the average compend, which, in the present state of medical education, is perhaps a necessity. The manner of placing the physiological actions and therapeutic uses of drugs in adjacent columns, and the frequent bracketed annotations of the editor seem to us an unfortunate arrangement, and, until one is accustomed to it, decidedly distracting. The most useful sections are the introductory chapters on general rules for prescribing and the appendix containing the formulary issued by the American Pharmaceutical Association. From the latter the practitioner may glean many suggestions valuable in writing his prescriptions.

Dr. Woodbury has done his work well and thoroughly, and in no instance will the American reader be led astray by English authorship.

SOCIETY PROCEEDINGS.

PHILADELPHIA COUNTY MEDICAL SOCIETY.

Stated Meeting, September 11, 1889.

THE PRESIDENT, W. W. KEEN, M.D., IN THE CHAIR.

DR. FERDINAND H. GROSS read a paper on

HÆMATOMETRA.

The rarity of hæmatometra is so well known that, while I have nothing new to say on the subject, I have been induced to report to the Society in some detail two cases of this character, which came under my observation during the early months of the current year. Atresia of the vagina I have, of course, frequently observed, but not when it was complicated by the state of which I now speak.

In the cases to which allusion has just been made, the atresia existed in one, at the lower end of the vagina, while, in the other, it consisted in an obliteration of the cervical canal of the uterus; and thus two interesting varieties of the occlusion were exemplified.

A young girl who had been under the care of different physicians without the source of her troubles being fully recognized, was referred to me by a medical friend for treatment, with the information that a tumor occupied the vagina and filled up the lower part of the pelvis.

The patient was in her fourteenth year, and therefore at the age of puberty; but she had never menstruated—that is to say, no visible sign of the catamenial flow had been noticed. The first disturbances of her health had occurred about six months before, and although she never felt quite well afterward, her indisposition, for a time, caused no apprehension. Finally, a number of paroxysms of great severity attracted attention, and the alarming character of the exacerbations led to a physician being consulted.

When, in an advanced stage, the case was referred to me, a digital examination of the vulva revealed protruding therefrom a smooth and very tense swelling, in which the experienced touch could readily detect fluctuation, particularly by rectal exploration. Neither the examining finger nor the probe could find anywhere between this protrusion and the labia an entrance into the vagina. The hypogastrium had become prominent from a tumefaction which arose from the pelvis. This enlargement in the course of its development had been noticed to be most sensitive during the above-mentioned paroxysmal attacks, which were doubtless the regular menstrual molimina. The continuous abnormal pressure upon the pelvic organs was the occasion of vesical

and rectal tenesmus as well as constipation; and restless nights were the inseparable accompaniments of such torments. Aside from these there were symptoms of a subjective character, such as headache, giddiness, nausea and other gastric distress, a feeling of painful fulness in the abdomen, palpitation, and disturbed vision which the prescribed glasses of the oculist had failed to relieve. The presence of such distressing and long-enduring symptoms naturally led to the diagnosis of hæmatometra as a consequence upon atresia hymenalis.

It is not my purpose here to recount with exhaustive minuteness the various results of hæmatometra if relief be not given in good time, by what is usually a simple surgical operation, nor would the time allotted permit me to speak of the lesion as it occurs in double vagina, in duplex uterus, and in other malformations of the female generative organs. Greulich relates in concise form, but with sufficient clearness, the different issues of this condition, and cites an array of authors who appear in the literature of the subject. Some of the results of these blood-tumors should not remain unnoticed in this place. Much may depend upon the period of life at which the atresia has been acquired. For example, in the climacteric age the hæmatometra may eventually be tolerated (*Real Encyclopædie der gesammten Heilkunde*, vol. vi. page 180), or at least borne with less suffering or discomfort, since at this period of change the tumor may cease to enlarge because of the discontinuance of the menstrual secretion. Again, at periods prior to the menopause, the rapidity or slowness with which the liquid accumulation takes place, and consequently the degree of its evil effects upon the general organism, may depend upon the character of the patient's constitution, whether this be plethoric and robust, or weakly and anæmic. If the latter be the case, the increase of the pent-up menses may be very meagre or even *nil*; and again, if vicarious menstruation be established, the addition *in utero* would naturally be voided, affording a degree of local relief, or, at least, checking for a time the progress of serious symptoms, provided the locality of the vicarious discharge be a safe one.

Among other possible results of this bloody accumulation are enumerated hæmatosalpinx, intra-peritoneal hæmatocele, and hæmatoma of the ovarium, each with its serious consequences. The internal *ostium* of one or both Fallopian tubes may be drawn open by the expansive force of the womb's accumulating contents, which, if not met by tubal obstruction, may ooze out at the *ostium abdominale*, and give rise to an intra-peritoneal hæmatocele and peritonitis. In hæmatoma of the ovarium the formative process is likely to be different. Whether at certain periods of functional activity, when the fimbriæ of the tube are said to clasp the ovary, to receive the ovule from the Graafian vesicle; or whether, at the time of direct communication between the tube and ovary, the fluid of the hæmatometra ever escapes into the bursting vesicle or stroma of the gland, must be left to hypothetical speculation. But hæmatoma of the ovarium as well as effusion into the tube may occur by direct extravasation from congested vessels and yet hæmatometra be responsible for either occurrence, since hyperæmia of the uterine appendages is one of the accompaniments or conditions of the hæmatic tumor we are considering. If, therefore, the already engorged plexuses or network of vessels of the environment be periodically subjected to additional blood-pressure, the bleeding that follows the bursting of a Graafian follicle under normal circumstances may now become sufficiently copious to produce results of a pathological character.

Without stopping to speak of a possible rupture which would allow the secretions to flow off in a natural or harmless direction, adhesions may form with the neighboring hollow organs, and perforation take place into bladder or rectum. Septicæmia from decomposition within the womb is also a great danger. The entrance of urine on the one hand, or of fecal matter or intestinal gases on the other, would be favored by the womb's enfeebled contractile power, as that organ could not empty itself with promptness nor with that degree of force of which it is possessed when developed for a physiological purpose. The pressure of tumors is recognized as a cause of uterine atrophy, and we can readily conceive in hæmatometra that an attenuated condition of the muscular coat might ensue. Another danger of infection may be a vaginal *cul-de-sac* below the point of perforation into either of the hollow viscera mentioned, which would serve as a reservoir for decomposed matter.

Considering the possible results of this condition, no time is to be lost in providing for the escape of the accumulated secretions.

Hymen imperforatum in a large majority of the cases is not discovered before the age of puberty, and the one referred to was doubtless of the congenital variety.

The operation can hardly be called a painful one, but the girl had become so irritable and sensitive from long suffering and repeated examinations, that the puncture by trocar was made under ether. Thirty-two ounces of reddish-brown, chocolate-colored fluid were drawn off without interruption of flow. The canula being removed, a crucial incision with a probe-pointed bistoury enlarged the opening, from which a little fluid oozed. Ergot was administered, but only gentle pressure was applied externally by an abdominal binder. The vagina was gently washed out with a weak solution of the bichloride. The uterus remained too high to be reached by the finger, and an examination by force was uncalled for. The vaginal surface was smooth and devoid of rugæ. As an additional precaution, the external parts were covered with compresses of antiseptic gauze.

It is to be remembered that the imperforate hymen is not a normal but a patho-anatomical structure. On the inner side its mucosa is continuous in cul-de-sac form with that of the vagina, and externally the mucous membrane is continuous with that of the vulva, while between these layers lies a fold of connective tissue. The membranous barrier in this case was found thick and tough beyond expectation, imparting the sensation of soaked leather to the fingers.

The relief was prompt and decided; on the second day there was some pain in the hypogastrium, but no significant rise in the patient's temperature. In four weeks the menses were discharged in a normal manner, and this has recurred at regular intervals ever since.

In the second case alluded to, the woman is close upon forty years of age, and has lived in the marriage relation for half that period, but remains childless. The evidence she relates as pointing to a miscarriage in the third year of her married life, is not conclusive. After that time she enjoyed good health until thirteen years

ago, when she "had a severe fall backward," after which she endured so much pain in trying to void her urine that she fainted. Her physician said she had "dislocated her womb," and used instruments to replace it. She was confined to bed for three weeks and then treated in a hospital in this city; whence she returned home, still in a feeble condition, and remained in poor health for several years. These early troubles are briefly mentioned since they concern the genito-urinary organs, and I will not tax your patience with a history of subsequent maladies which had but doubtful or no connection with the patient's later complaint. Suffice it to say, that after an attack of typhoid fever as far back as 1883, she regained her former good health, and in all the years that followed until the month of May, 1888, the menstrual function was regularly performed. It was then, however, that the lesion which concerns us here appears to have had its beginning. The monthly discharge of blood became scanty, and to the patient it appeared as though "a stringy leucorrhœal discharge had replaced the regular flow." Nor was she as free from pain as she had formerly been during her regular periods. In the following August her menses failed to appear, and the amenorrhœa continued for eight months; that is to say, until she was relieved by the operation mentioned further on. As regular as had been her monthly periods when in health, just so regular were they during these eight months when the attacks of lumbar pains and uterine cramps presented themselves, the severity of which increased with each returning molimen. In vain she now applied for relief both to regular and irregular practitioners. The latter advised her to enter their hospital and submit to a laparotomy for the removal of what they pronounced to be a "fibroid tumor attached to the uterus and left ovary." This advice was not followed, and it need hardly be said that the diagnosis was as incorrect as the laparotomy would have been unwarrantable. The condition grew from bad to worse. From dire necessity the woman had learned to catheterize herself and was enabled by that means to obtain frequent relief from one of her greatest torments.

On the 30th of last March she entered the German Hospital, where she became my patient. On examination the next day, I found a large, smooth, very hard and firm tumefaction pressing low in the pelvis. The os could not be found and there was no discernible discharge from the vagina. No fluctuation was perceptible, either by vaginal or rectal examination. The mass had repeatedly been held by others to be a fibroid; but on hearing the patient's own account of the case, from which the above is a condensed statement, I concluded it to be a hæmatometra, at once verified the diagnosis with an aspirating needle, and on April 2d introduced an ordinary sized trocar and drew off the long pent-up secretions. In penetrating at the indistinct but supposed point of the former os, the instrument imparted the sensation of passing through a wall of considerable thickness before reaching the cavity, showing the obstruction to be something more than membranous. This was probably brought about by a stenosis of the inner os, combined with an endocervicitis and adhesions of the entire cervical canal.

The mahogany-colored, jelly-like fluid, or semi-fluid, could not flow freely through the canula, but came only drop by drop. The cervix dilator therefore replaced the canula, and being used as a director for making a shallow crucial incision, a quart of the tarry substance was drawn off. The cavity of the womb was freely washed out with a continuous stream of the mercuric solution. Ether was not administered. The after-treatment was in the main similar to that instituted in the other case. Recovery was complete, and the patient has menstruated at regular periods several times since the operation.

DR. JOHN C. DA COSTA asked Dr. Gross where he punctured the uterus in the second case? Did he attempt to follow the cervical canal, or did he put the trocar directly into the mass? He congratulated both patients on having fallen into the hands of Dr. Gross, and out of the hands of their previous advisers. Hæmatometra is not of common occurrence, and when we have it, it is usually due to closure of the os, or atresia, so called. The first case reported would, he thought, be found described in the books rather under the head of atresia of the vagina than of hæmatometra; neither could he understand how those who had previously examined and treated the case could have overlooked the nature of the difficulty. They would not have done so if they had heard the late Professor Wallace lecture on atresia vagina, as he used to describe it, with "the bulging membrane looking like a child's head coming out." If they could not have made a vaginal examination, an examination by the rectum would have shown the uterus above and the soft fluctuating mass below, and made the diagnosis for them.

The second case seemed to be due to closure of the cervical canal. This is more common after the application of solid nitrate of silver than people generally are aware. There is apparently something particularly vicious about the application of solid nitrate of silver to the cervical canal. Stronger agents may often be used without producing the trouble it does. The speaker was unable to see how in this case a practitioner could mistake the nature of the trouble. The gravity of such cases often arises from imperfect examination of the parts, and, therefore, ignorance of the condition, and as a result of this—want of timely treatment.

DR. LAWRENCE WOLFF had had the pleasure of examining the second case with Dr. Gross. He had assisted him in emptying the uterus, and his curiosity having been aroused as to the nature of the chemical changes that had taken place, he secured some of the contents for examination. The microscope showed that there were no corpuscles present. He made the blood test, and secured Teichmann's hæmatin hydrochloride crystals. While there was undoubtedly blood, there was no hæmoglobin, but hæmatin. He thought the question is not altogether solved as to what becomes of the serum-albumin in such conditions. The uterus is not prone to take up serum-albumin, but here it had evidently been absorbed, and nothing but globulin and hæmatin remained, as was found to be the case when the mass was mixed with water, which rendered it turbid, the globulin being insoluble in water. The fact that globulin is not taken up is not generally known. We know that the corpuscles, as a result of pressure, poisons, and other conditions, will break down, and that hæmoglobin will be changed to hæmatin, but it is not generally understood that in hæmatometra serum-albumin, which is diffusible, is reabsorbed and globulin retained. These facts have a bearing upon various pathological conditions, especially

some forms of nephritis, and that is one reason why he had brought up these points in regard to the condition of the blood in these cases.

DR. JOSEPH PRICE said that these congenital forms of atresia were quite common. In the last few years a great many operations have been performed for infantile forms of the female generative organs, and in many of these cases we find atresia. In the last year he had done two sections for regular menstrual molimen without visible flow. The patients had suffered from the age of thirteen or fifteen years to thirty-five, without any menstrual fluid showing itself. In these cases atresia with infantile uteri was found. The appendages were rudimentary. There was no accumulation. In both instances the results have been quite happy.

A few years ago, a lady asked Dr. Price to attend her in her confinement. Conception had immediately followed an operation upon the cervix uteri done by an estimable operator. She had been sterile for five years before the operation. Previous to this she had borne three children. She sent for him after she had been in labor for some time. There had, however, been no show. On examination he found a supra-vaginal diaphragm, without the semblance of os or cervix. He made a careful speculum examination, and sent for a friend, a man of good judgment and experience. The head was engaging, but no os could be found. A crucial incision was made in this diaphragm, and she was delivered of a healthy child without trouble. She made a good recovery, and has since borne children. In this case, although a canal had been left, there were probably some granulations which subsequently closed the channel.

Ten days ago he operated upon a cicatricial case. The woman had borne four healthy children, but had not menstruated for some years. He found but little accumulation. There had been extensive sloughing, and he had to make a vagina throughout. A portion of the blood had apparently been absorbed. The material found in these cases is usually of a hard character similar to that found in hæmato-salpinx. It will make a black mark, and can be used as a crayon.

The discharges from the tubes of which Dr. Gross spoke, he was satisfied last only a short time. The irritation is sufficient to provoke occlusion of the tube, and we have the clubbed condition of the tubes seen in accumulation of fluid in the tubes. The tubes look like the end of an amputated finger. I have here two specimens illustrating this point. The woman from whom this was removed never conceived. There have been salpingitis and local peritonitis, causing closure of the pavilion. Here we have a broad ligament cyst, an occluded tube, and a typical hydrosalpinx. Schœder gives an interesting discussion of this entire subject, and cautions care in the examination that leakage or rupture may not occur. It is evident from his remarks that this accident had occurred in his hands.

DR. GROSS, in reply to the question of Dr. Da Costa, stated that there was only a point indicating where the os might have been. He followed this in the direction one would suppose that the canal would take. It was some time before the trocar reached the cavity.

In regard to the first case, an examination *per rectum* might have revealed a small uterus if the fluid had been contained only in the vagina. This case was, however, of six or seven months' standing, and the secretions, after filling the vagina, had also dilated the cervix and uterus. Here the term hæmatometra is also used. When the vagina only is filled, it is an hæmatocolpos. To be more exact, unless we object to long names, the term hæmatometrocolpos would express the condition when both vagina and uterus are involved.

Dr. Gross was obliged to Dr. Wolff for his interesting remarks in regard to the character of the fluid, which made the report of the case more complete.

OBSTETRICAL SOCIETY OF PHILADELPHIA.

Stated Meeting September 5, 1889.

JOHN C. DA COSTA, M.D., IN THE CHAIR.

DR. JOHN C. DA COSTA read a paper entitled,

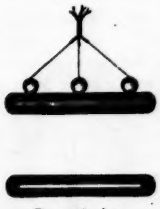
AN EASY METHOD OF REPAIRING THE PERINEUM.

He did not claim anything new, the operation being the result of a combination of old ideas, and an easy and simple method, answering equally well whether the tear is long or short.

Mr. Lane, of London, in 1860, used ivory bars with small perforations for approximating the edges of the perineal tear, and reported thirty consecutive cases without a failure. Dr. Thompson, of Washington, used flat rubber bars with small holes in them, and has reported fifty-three consecutive cases, all cured. Dr. Thomas, after speaking of the quill suture, leads us to infer that he uses perforated bars, and states that he does not recall a failure in his experience.

The operation which Dr. Da Costa recommended is very simple and the instruments are few: a pair of blunt-pointed scissors, a perineal needle, a little silver wire and a few shot, a shot-compressor, and two bars shaped like the accompanying cut:

FIG. 1.

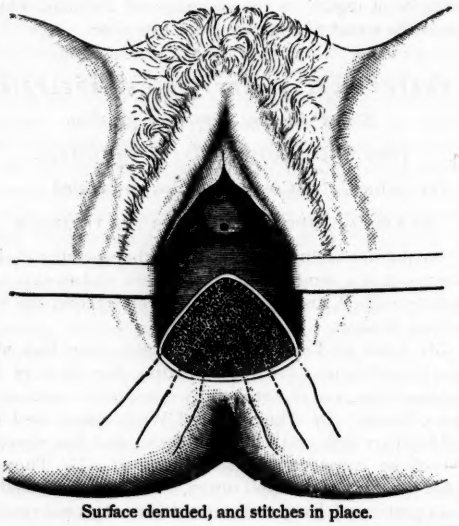


Bar, ½ size.

The operation is begun at the bottom of the tear in the vagina. With one or two fingers in the rectum, a little slit is made at the lowest point and the torn tissue is denuded. It is important to get rid of all scar tissue, for unless this is done good union will not be secured. After freshening the bottom of the tear, the scissors are turned to the right and to the left, and each side is denuded. Then, with four cuts of the scissors, the loosened cicatricial tissue is removed. The first stitch is made near the bottom of the raw surface and is passed three-fourths of an inch from the edge of the cut portion, buried the entire distance, and comes out on the other side equally far from the border of the wound. The needle is then threaded with silver wire and withdrawn. The second stitch is put in in the same way. The third stitch is started in the skin like the others, three-fourths of an

inch from the edge of the cut, and carried along just under the edge of the freshened surface the whole way around. This is the most important stitch of all, and was introduced by the late Albert H. Smith, when one of the physicians-in-chief at the Nurses' Home some years ago. The stitches are buried throughout, and only three are used in the operation. All that is necessary is to bring them out in a straight line.

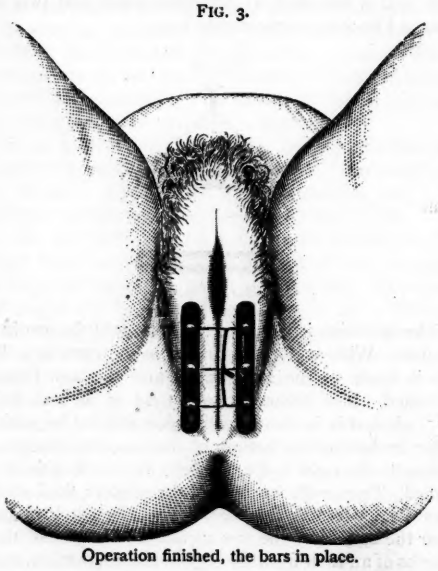
FIG. 2.



Surface denuded, and stitches in place.

After this, the wires are slipped through slotted rubber bars, on each side, and shot clamped on them. After the shot are clamped, the ends of the wires are twisted

FIG. 3.



Operation finished, the bars in place.

over the median line, and passed through a piece of catheter. Any dressing may then be applied.

The advantages of this operation are many. In the first place, but three stitches are used. Probably every gynecologist has seen cases where there has been deep quilting, and where the tissue has sloughed out because the circulation has been so interfered with that nutrition could not be maintained. These three sutures interfere very little with the circulation, and they hold together the deep parts of the wound. When inflammation takes place, the wires over the shot are cut; the bars spread and relieve the tension and prevent any tendency to sloughing, while still supporting the parts.

This is a different operation from that in which the denudation is made in curved lines, and where another operation is required for any existing rectocele. The operation described above will include a rectocele. It is better than another popular operation, which does not restore the triangle which nature made, but makes a beautiful skin-flap, which looks well from the outside, but affords no support.

DR. JOSEPH PRICE said that there were a few points about which he would like to speak in connection with this operation. As Dr. Da Costa had pointed out, this is an old operation, and can be found in all the text-books. It is the old operation upon the posterior wall, and has the merit referred to, of, in many cases, making a superficial or skin perineum. The principle of suturing described is not adopted in any other branch of surgery, and he was sure Dr. Da Costa would not apply this principle to any other portion of the body. He said that sometimes he denudes a distance of three inches. In no other part would he approximate such a surface with three sutures, as three sutures will not close it.

One of the illustrations shows what takes place in many perineal tears. The skin-perineum side is not harmed; but if a finger is placed in the sulcus on one side, a sense of resistance is found which is absent on the other side. The sulcus is a deep one, and is a lateral tear. As has been remarked by Dr. Deaver, "It is like the lateral cut for stone." In such a case the operation is almost a unilateral one. It is just such a state of affairs that Emmet had in view in his classical operation for the restoration of the pelvic floor, and he has been wonderfully successful. The needle used should be very small.

DR. DA COSTA, in reply to Dr. Price, said that the criticism made in regard to one of the illustrations had nothing to do with the subject under discussion. If there is a line of cicatricial tissue on one side, we do not need to denude both sides to repair the condition, for it is a simple matter to remove the scar tissue and sew it up, as in any other surgical operation.

DR. JOSEPH HOFFMAN made a

REPORT OF A SERIES OF ABDOMINAL SECTIONS, WITH SPECIAL REFERENCE TO COMPLICATIONS.

The list of operations may be classified as follows: one strangulated ventral hernia; one appendicitis and hæmatocele; sixteen cases of ovarian and tubal disease with adhesions, inflammation, and occlusion of the tubes, with one death, the result of sepsis from a miscarriage brought on instrumentally. In other words, he believed the tubes were in a septic state, and gave rise to the peritonitis.

In two cases the gonorrhœal origin of the pyosalpinx was well established; in two the history pointed to a post-puerperal origin; in the remaining two the origin was

doubtful, though in one of the cases specific trouble was strongly suspected.

One case died of shock—never coming out of the anæsthetic. She was a hard drinker.

In two cases the tumor removed was a dermoid cyst, both of which were small. In one of these cases the uterus was rudimentary, though the woman had for a long time worn a pessary for a so-called displacement, introduced by a specialist in gynecology.

In two cases exploratory incision was made, and the women recovered quickly. One of these died soon after tapping; from what cause he did not know. The second exploratory incision was due to an error in diagnosis. The uterus was retroverted, a miscarriage having occurred a short time previously. There was a peculiar thickening of the right broad ligament, which led to the blunder.

In the thirty-three operations recorded, two deaths have occurred. The first occurred early in the series. In the last twenty-six cases there has been but one death, and the last eighteen cases have been without a death.

DR. J. PRICE said that the mortality of this group of cases was exceptionally low. The pus cases and the extra-uterine cases interested him the most.

DR. JOSEPH PRICE reported

TWO OPERATIONS FOR EXTRA-UTERINE PREGNANCY.

The first case was that of a white woman, aged thirty-five, nursing a child of thirteen months. The menses appeared on the fourth month of lactation, and remained perfectly regular at intervals of twenty-seven days with four days' duration; finally, they were absent two periods, and paroxysms of pain and collapse asserted themselves. He immediately operated for ruptured tubal pregnancy, and found about a quart of clotted blood in the peritoneal cavity; tubal rupture on the left side, and hydrosalpinx on the right side. These were removed, and recovery occurred.

The second case occurred in a negress, and is of great interest, as ovarian cysts are very rare in true Africans, and microscopical examination gave a doubtful result, although the growth resembled one very closely. One point of great interest in connection with these cases that survive the rupture and go into the hands of the surgeon, is the marked difference in the character of the hemorrhage from those that go into the hands of the coroner—and they are numerous. In the latter cases the hemorrhage is overwhelming, and the abdomen is found full of blood. The surgeon finds probably one-fourth that amount of blood. Dr. Formad, the coroner's physician, states that in one case he found the peritoneum deluged with blood, and the fœtus washed up on the pancreas.

DR. J. M. BALDY said, though Dr. Price had stated that ectopic gestation is rare in the full-blooded negro, that he should like to place on record such a case. The woman had been having children at regular intervals of two years for eight or ten years. The last time, when her time came to become pregnant, it proved to be a pregnancy of the tube. She was operated on and saved.

DR. J. M. BALDY reported a case of

FIBRO-CYSTIC TUMOR OF THE UTERUS.

Mrs. A., aged thirty-five years, married, no children. She had a lump in her abdomen for fifteen years, which remained quiescent until within the last two years, since which time it has grown rapidly. The menses gradually became irregular and profuse; bowel and bladder symptoms were severe; pus appeared in the urine; the abdomen was constantly swollen, and very painful; and the general health had begun to suffer severely. Examination showed a uterine tumor, and its removal was advised, the dangers of the operation being fully explained. The operation was eagerly accepted. The tumor was removed, and proved to be an extremely nodular fibroid, which had undergone cystic degeneration in part, and in other parts was quite œdematous. The mass was firmly wedged into the pelvis, and was delivered with the greatest difficulty, leaving practically no pedicle at all. The case was treated by supra-vaginal amputation, a wire nœud being first placed around the lower portion. The stump was treated by the extra-peritoneal method, as advocated by Bantock. After the tumor had been cut away, there was left a stump with a diameter of over three inches; this was gradually trimmed away until it was reduced to about an inch and a half in diameter.

This case presents the opportunity for a few remarks on the method of treating the pedicle in hysterectomy, and on the use of electricity in fibroid tumor of the uterus.

There are two methods of treating the stump — the intra-peritoneal, as advocated by Martin, and the extra-peritoneal, as advocated by Bantock. All other methods devised or proposed are simply modifications of these two, and are far from being as good as the originals.

What is wanted are results, and in questioning different gentlemen who are experimenting with the so-called improvements in hysterectomy, it has been found almost universally that their losses amount to from thirty per cent. to fifty per cent. The patients who get well may do so quickly, and the operations may be very beautiful, theoretically, but the results are murderous! The results obtained by the extra-peritoneal method are, at present, very decidedly the best, and have proven eminently satisfactory in his hands.

Martin, by the intra-peritoneal method, reports a series of eighty-four cases, with twenty-five deaths. Later, he had thirty cases, with three deaths; and still later he had "another series with good results;" and last, "a series with bad results." And so, after an experience of over 125 cases, he ends up with a series which is so bad that he does not publish it. In contrast with this stands Bantock's record, by the extra-peritoneal method, of fifty-seven cases with only twelve deaths, and his results continually getting better to the end. He now has had a run of thirty or forty cases without a death. These figures speak for themselves. After all his experience, Martin says "we must wait for a larger number of cases before deciding this question."

Of the twenty-five deaths met with by Martin in his first eighty-four cases, fifteen died of "bleeding, embolism, and collapse," all of which, of course, means hemorrhage. By the extra-peritoneal method, these would all have been saved, as bleeding cannot possibly occur if the wire does not slip or break. Again, ten of Martin's cases died of sepsis; this also is much less likely to happen by the extra-peritoneal method, as all cut surfaces are outside the peritoneal cavity, in plain sight, and under perfect control.

When Martin has finished his operation and drops the

stump, the appearance to the naked eye is simply perfection. On the other hand, when the stump is left outside, as is taught by Bantock, the after-treatment is often tedious, and the convalescence prolonged. If the stump is not perfectly dry, it is apt to suppurate, and, at best, it is an unsightly affair. But when we contrast the results, there can be but one choice, if we give proper consideration to the safety of our patients. Not only are Martin's own results bad, but in the hands of less expert and experienced operators the mortality is very high. Even Bantock has lost four out of five cases by the intra-peritoneal method.

CORRESPONDENCE.

A SIMPLE METHOD OF TREATING UMBILICAL HERNIA IN INFANTS.

To the Editor of THE MEDICAL NEWS,

SIR: The appliance described under this heading in the number of THE NEWS for October 5th is very similar to one which I have successfully used for twenty-five years past, and which originated with myself. It fulfils the requirements stated to be essential—it is "non-absorbent, non-irritating, and of such material that it will remain in place for at least a week, notwithstanding daily bathing." It has one great advantage over the "hard-rubber, slightly oval, plano-convex lens with two wire loops," and is readily attainable everywhere and at all times. I use the large removable head of a common picture-nail. This is applied to the hernia and fixed in place by a strip of adhesive plaster. My first attempt was with the half of a toy marble. The nail-head is better because larger and less convex. This simple appliance is at command everywhere, and I only thought it worthy of presentation to the profession when I read the article alluded to above.

Very truly yours, J. C. REEVE.

DAYTON, OHIO.

NEWS ITEMS.

The Moral Sense of a Physician.—Those members of the profession who believe in the condition known as "moral imbecility," will have their opinions greatly strengthened by the following letter received by the London *Lancet* in reply to an indignant protest made by that journal concerning Dr. Allwright's plagiarism of Dr. Reeve's article in Hirst's *System of Obstetrics*:

"*To the Editors of The Lancet*—SIRS: In answer to your favor, the article mentioned obtained its foundation from the American *System of Obstetrics*, but it was not intended to be a copy, nor, I think, is it. An article in Buck's *Hand-book of the Medical Sciences* contains similar facts upon the same subject, as, indeed, must any other history on the same subject. I am very sorry if an objection has been raised to the article appearing in print. I have practised both the method of administering chloroform and also that of ether, which are suggested in the article, for some time. Perhaps you will offer my apology, and that any offence against Dr. Hirst was unintentional. I am, sirs, yours obediently,

"FRED. W. ALLWRIGHT."

"BRIGHTON, Sept. 29, 1889"

College of Pharmacy.—Another college of pharmacy is to be located at Brooklyn, New York, controlled by the Pharmaceutical Society of King's County, which has held the matter under advisement for some time in the hands of an influential special committee. It has been satisfactorily demonstrated that there are enough young druggists from Brooklyn attending the New York colleges to form a strong class, if they can be shown that a home institution has been founded that is worthy of their support.

A Maritime Medical Association.—At the last annual meeting of the New Brunswick Medical Society, held at St. John, a plan was favorably considered for the bringing about of a union between the societies of the provinces of Great Britain lying eastward from Quebec Province. A committee of conference was appointed, and another year will probably see the formation of a strong and influential Maritime Medical Society, combining the membership in Nova Scotia and New Brunswick, and attracting to it many physicians who have not hitherto associated themselves with any organization.

The Dedication of Clark University.—The dedication of the new university at Worcester took place on the 2d instant, and was a memorable occasion. It marked the landing of a puritanical group of educators in the Bay State, and yet "Severe Science" is the inscription on the standard set up by Dr. Stanley Hall, the president and inspirer of the new departure. It is the first University of the Basal Sciences which has made its appearance in America: these basal subjects being five in number, namely, Psychology, Biology, Physics, Chemistry, and Mathematics. To medical students who intend to obtain a good foundation, this will prove attractive by reason of the advantages to be found here in biology, physiology, neurology, anatomy, and chemistry. Dr. Stanley Hall, in his inaugural address, quoted an eminent European physician as his authority for the opinion that the young man who hurries on to the more attractive fields of the clinic and the amphitheatre, to the neglect of the more abstruse studies, is in the position of the prodigal who becomes prematurely old and impotent. Dr. Hall uses the time-worn adage, "Godlike is the physician who is also a philosopher," and claims that this proverb is far more true to-day than when Hippocrates framed it.

The Continental Anglo-American Society.—The Continental Anglo-American Society, which has recently been formed, held its first meeting in Paris on the 30th of September, Sir Spencer Wells occupying the chair as one of the Honorary Presidents. Among those present were Drs. Alan Herbert, Faure-Miller, Thomas Linn, and others, of Paris; Dr. Sandwik, of Cairo; Dr. Stanley M. Rendall, of Aix-les-Bain, and many others. In the evening a dinner was given at the Grand Hotel. Among the guests were Dr. Norris, of New York, and Dr. Rodgers, of Illinois.

The Society has been organized chiefly through the endeavors of Dr. Thomas Linn, the Honorary Secretary, and already has about seventy-five members.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF THE MEDICAL CORPS OF THE U. S. NAVY, FOR THE WEEK ENDING OCTOBER 12, 1889.

BRIGHT, G. A., *Surgeon*—Detached from temporary duty at the Naval Academy, and placed on waiting orders.